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CONTENTS

PAGE

E. KNIGHTING, D. E. JONES and M. K. HINDS—Numerical experiments in the integration of the meteorological equations of motion 91

R. M. GOODY and W. T. ROACH—The determination of tropospheric ozone from infra-red emission spectra 108

E. K. WEBB—Vanishing potential temperature gradients in strong convection .. 118

J. OLIVER—William Bulkeley's record of the weather of Anglesey 1734-1743, 1747-1760 126

V. E. SUOMI, D. O. STALEY and P. M. KUHN—A direct measurement of infra-red radiation divergence to 160 mb (Plate II) 134

R. A. HAMILTON—The meteorology of Northern Greenland during the midsummer period 142

Shorter Contributions—

R. R. LONG—Tractable models of steady-state stratified flow with shear 159

E. J. SMITH, K. J. HEFFERNAN and W. J. THOMPSON—The decay of the ice-nucleating properties of silver iodide released from an aircraft 162

J. R. SCOTT—An essay on low-latitude wind analysis 166

J. GLOVER and J. S. G. MCCULLOCH—The empirical relation between solar radiation and hours of sunshine 172

R. H. A. STEWART—Summary of observations at Shackleton, Antarctica 1956-57 .. 176

Correspondence—

A. E. PARKER; J. CLODMAN—Anisotropic high-level turbulence 180

R. S. SCORER—Airflow over mountains : indeterminacy of solution 182

G. REYNOLDS; S. GREGORY—Annual rainfall probability maps of the British Isles .. 184

Discussions—

H. RIEHL and D. FULTZ—Jet streams and long waves in a steady rotating-dishpan experiment : structure of the circulation (*Q.J.*, **83**, p. 215) 186

G. B. TUCKER—Evidence of a mean meridional circulation in the atmosphere from surface-wind observations (*Q.J.*, **83**, p. 206) 187

J. L. MONTEITH—Dew (*Q.J.*, **83**, p. 322) 188

N. E. RIDER—Water losses from various land surfaces (*Q.J.*, **83**, p. 181) 190

B. J. MASON and D. K. GHOSH—The formation of large droplets in small cumulus (*Q.J.*, **83**, p. 501) 193

Proceedings at meetings of the Society 195

Reviews—

Exploring the atmosphere's first mile, Eds. H. H. Lettau and B. Davidson (F.P.); *'Meteor' Expedition, 1925-1927*, G. Wüst (J.N.C.); *The threshold of space*, Ed. M. Zelikoff (R.M.G.); *Light scattering by small particles*, H. C. van de Hulst (D.D.C.); *Atmosphärische Elektrizität*, H. Israel (J.A.C.); *Vom grossen Eiszeitalter*, E. Ebers (G.M.); *Precipitation, temperature and sultriness in Africa*, K. Knoch and A. Schulze (G.A.B.); *Solar control and shading devices*, A. Olgyay and V. Olgyay (P.P.) 197

Obituary Notices 201

Corrigendum 204

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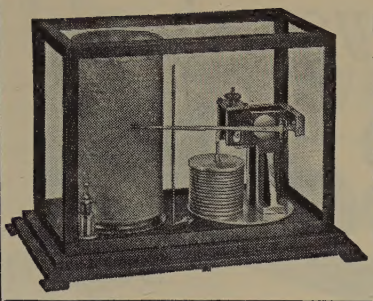

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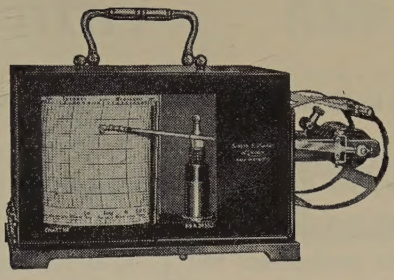

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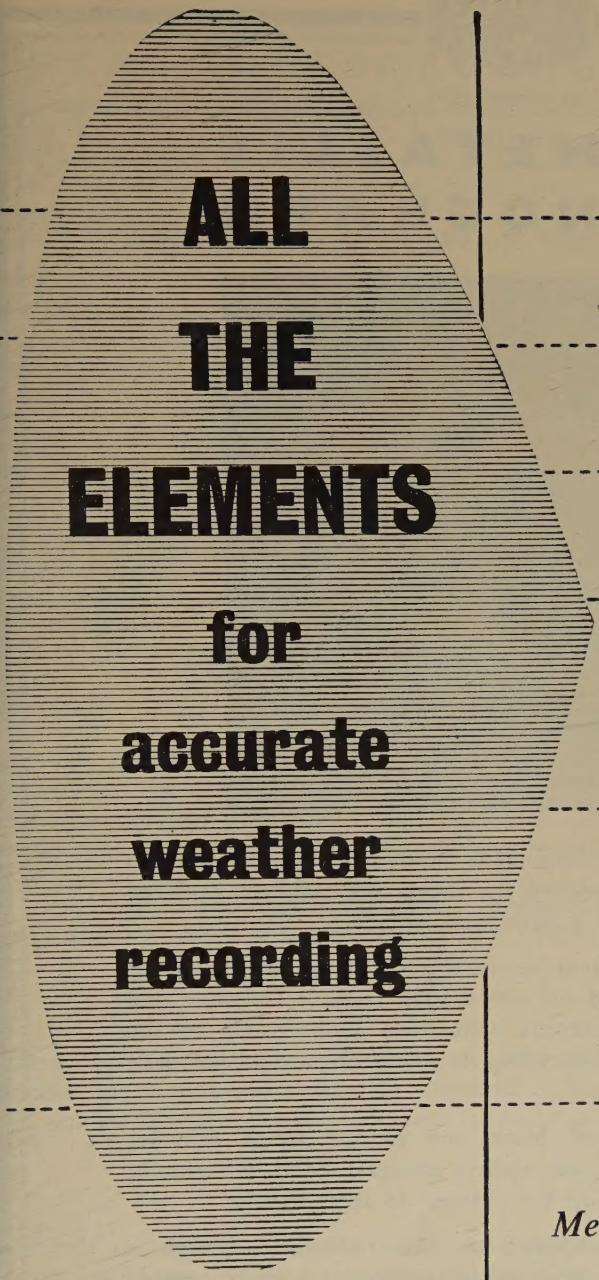

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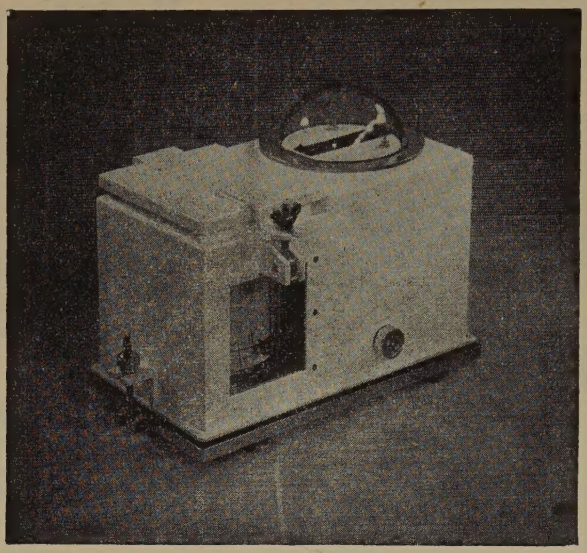

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Numerical experiments in the integration of the meteorological equations of motion

By E. KNIGHTING, D. E. JONES and MAVIS K. HINDS
Meteorological Office, Dunstable

(Manuscript received 10 September 1957)

SUMMARY

It is usual to use finite-difference methods in carrying out the time integration of the equations of motion as adapted to the prediction of the large-scale atmospheric pattern, and this implies a choice of the mesh length and time increment to be used. The effect of changing the size and orientation of the mesh upon the computed values of the vorticities and their advection is examined, and the resulting differences between 24-hr predictions are shown to be important.

1. INTRODUCTION

The so-called 'vorticity equation,' relating changes of the vertical component of vorticity to the advection and divergence, forms the basis of all practical methods of numerical weather prediction so far developed. In particular the barotropic equation used in the simplest system of practical value is merely an expression of the conservation of absolute vorticity.

Hence in the numerical calculations involved in predicting the synoptic-scale flow pattern over a period of a day or so we have to deal with expressions of the form

$$\frac{\partial \zeta}{\partial t} + \mathbf{V} \cdot \nabla \zeta$$

where ζ is the vertical component of vorticity. Further, it has been found convenient to make use of the quasi-geostrophic character of the motion and assume that the velocity, but not its divergence, can be derived from the geopotential

$$\mathbf{V} = \frac{g}{f} \mathbf{k} \times \nabla h \quad . \quad . \quad . \quad . \quad (1)$$

where $\mathbf{V}$ is the velocity, h the height of an isobaric surface, g the acceleration due to gravity and f the Coriolis parameter; the gradient is taken in the isobaric surface. The vorticity, ζ, is then given by

$$\zeta = \frac{g}{f} \nabla^2 h \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The term $\mathbf{V} \cdot \nabla \zeta$ may be written, in view of Eq. (1) as

$$\mathbf{V} \cdot \nabla \zeta = \frac{g}{f} J(h, \zeta) \quad . \quad . \quad . \quad . \quad (3)$$

J denoting, as usual, the Jacobian so that the typical equation with which we deal is

$$\nabla^2 \frac{\partial h}{\partial t} + J(h, \zeta) = F(h, \zeta, f) \quad . \quad . \quad . \quad (4)$$

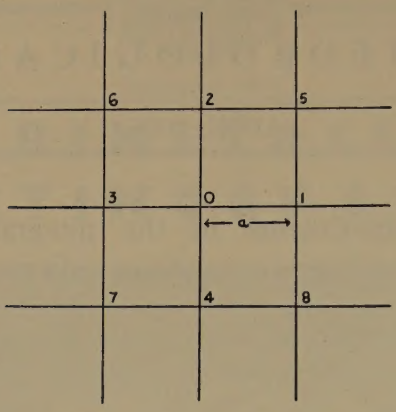

Figure 1. The numbering of the mesh points

Explicit analytic solutions of such equations cannot be obtained and the integration is carried out by replacing the equations by their finite-difference analogues in terms of the height of a pressure surface (or the thickness between two pressure surfaces) at the grid points of a square mesh. The initial values of h at the grid points are not observed, but are interpolated and even extrapolated values obtained from observations which are irregularly distributed. The finite-difference approximations to ∇^2 and J will depend upon both the length and orientation of the mesh, although the differential operators themselves are symmetrical. The object of this investigation is to compare the finite-difference approximations to ∇^2 and J using different mesh lengths and orientations and to find their effect upon the time integrations.

2. The basis of the computations

One finite-difference approximation to $\nabla^2 h$ is

$$\nabla^2 h = \frac{h_1 + h_2 + h_3 + h_4 - 4h_0}{a^2} \qquad (5)$$

where h_i is the value of h at the grid point number i in Fig. 1, a being the mesh length. A similar formula applies if we consider the points number 5, 6, 7, 8 instead of those numbered 1, 2, 3, 4 since ∇^2 is a symmetric operator. Thus another approximation is

$$\nabla^2 h = \frac{h_5 + h_6 + h_7 + h_8 - 4h_0}{2a^2} \qquad (6)$$

The factor 2 occurs in the denominator because we are now considering a mesh of side $\sqrt{2}\,a$ making an angle $\pi/4$ with the previous mesh. We have denoted quantities such as ζ calculated in these two ways by ζ^+ and $\zeta^\times$, ζ^+ being calculated from the cross formed by the points 0, 1, 2, 3, 4 and $\zeta^\times$ from the St. Andrew's cross formed by the points 0, 5, 6, 7 and 8. ζ^+ is the finite-difference approximation which is usually used.

The definition of the operator J is

$$J(h, \psi) = \frac{\partial h}{\partial x}\frac{\partial \psi}{\partial y} - \frac{\partial h}{\partial y}\frac{\partial \psi}{\partial x} \qquad (7)$$

and the usual approximation to J is

$$J(h, \psi) = \frac{(h_1 - h_3)(\psi_2 - \psi_4) - (h_2 - h_4)(\psi_1 - \psi_3)}{4a^2} \qquad (8)$$

the numbering being as in Fig. 1, so that J is a function of the values of h and ψ at the corners of a square. We may compute J from the values at the corners of any other square; thus an alternative approximation is

$$J(h, \psi) = \frac{(h_5 - h_7)(\psi_6 - \psi_8) - (h_6 - h_8)(\psi_5 - \psi_7)}{8a^2} \qquad (9)$$

and again we distinguish between these two finite-difference approximations by writing the first as J^+, the second as $J^\times$. When ψ is $\zeta + f$ we already have two alternative finite-difference approximations to it, $\zeta^+ + f$ and $\zeta^\times + f$, and there are four possible Jacobians

$$J^+(h, \zeta^+ + f),\ J^+(h, \zeta^\times + f),\ J^\times(h, \zeta^+ + f),\ J^\times(h, \zeta^\times + f)$$

and we denote these by J^{++}, $J^{+\times}$, $J^{\times+}$, $J^{\times\times}$ respectively. The usual methods of integration of the dynamical equations use J^{++}.

3. The calculations

The values of the vorticities and the Jacobians were calculated from the heights of the 500-mb surface over the mesh of 18 × 14 points in current use in the Forecast Research Division at Dunstable. This is a square grid on a polar stereographic projection with a grid length of approximately 160 mi in the latitude of the British Isles. It gives vorticities over a mesh of 16 × 12 points and Jacobians over a mesh of 14 × 10 points for ζ^+ and J^+, but with points missing at the corners of the other fields owing to the methods of calculation. The values which have been computed and which are presented in Table 1 are

$$\zeta = \frac{\beta^2 g}{f} \nabla^2 h \qquad . \qquad . \qquad . \qquad . \qquad (10)$$

and are expressed in $10^2\ \mathrm{hr}^{-1}$, β being the map magnification factor on this projection, so that a vorticity given as 064 is $0{\cdot}64\ \mathrm{hr}^{-1}$. The computed values of the Jacobian are

$$J_{\text{computed}} = a^2 J\left(h, \frac{\beta^2 g}{f} \nabla^2 h + f\right) \qquad . \qquad . \qquad . \qquad (11)$$

the results being expressed in ft hr^{-1} in Table 4, etc. These are the expressions used in the time integrations.

The calculations of vorticities and Jacobians were carried out for 18 cases which had been used to test the Sawyer-Bushby forecasting equations (Sawyer and Bushby 1953); these cases were used because the data required were readily available in a punched form for use in the electronic computer. It is impracticable to reproduce all the results, and in the Tables we have given the results for a single case which appeared to show wider divergences than the average and which has been used as a test case in several modifications of the Sawyer-Bushby model.

4. Results of the vorticity calculations

The result which is most apparent is that the field of $\zeta^\times$ is generally smoother than that of ζ^+ in regions where ζ is large, i.e., $|\zeta|$ is of the order of $0{\cdot}4\ \mathrm{hr}^{-1}$ which is the approximate value of the Coriolis parameter, f. The patterns are often similar, in that maxima of $\zeta^\times$ occur in the same regions as maxima of ζ^+, often at the same grid point, but the maxima of $\zeta^\times$ are usually less than those of ζ^+. The minima, often small negative values of the order $0{\cdot}05\ \mathrm{hr}^{-1}$, are more difficult to correlate and are probably of less importance. Occasionally ζ^+, or both ζ^+ and $\zeta^\times$ were negative and large so that $\zeta + f$ at a grid point was less than zero, and some suspicion must be attached to the analysis near such points, the absolute vorticity being negative; in the time integration negative values of $\zeta + f$ are made zero. If the height h was changed by an amount ϵ and this causes a change of ϵ' in $\zeta^\times - \zeta^+$ at the point, then the resulting changes in the field of $\zeta^\times - \zeta^+$ would have the pattern shown in Fig. 2. A difference of $\epsilon = 200$ ft leads to a value of ϵ' of $0{\cdot}80\ \mathrm{hr}^{-1}$. In several cases a pattern very similar to that of Fig. 2 was clearly discernible and examination of the original data showed that an error had been made in reading off the value of h from the synoptic chart. This property of the field of $\zeta^\times - \zeta^+$ is now used to test the initial data for use in a time integration to detect errors in interpolation; $|\zeta^\times - \zeta^+|$ is formed and tested to see if it exceeds $0{\cdot}30\ \mathrm{hr}^{-1}$.

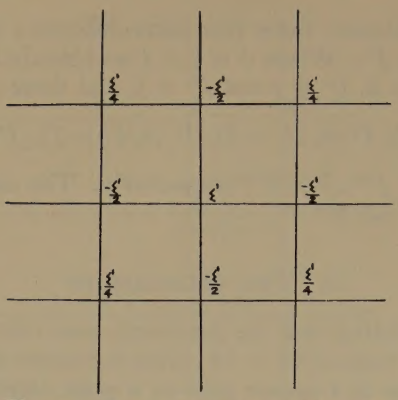

Figure 2. Error distribution of $\zeta^{\times} - \zeta^{+}$.

The computations based upon the data at 15 GMT 30 Jan. 1952 serve as an example of large vorticities and differences which are typical of the results found. The 500-mb contour chart is shown in Fig. 3 (a) while ζ^{+} and the differences $\zeta^{\times} - \zeta^{+}$ are given in Tables 1 and 2. Regions in which both $\zeta^{+} + f$ and $\zeta^{\times} + f$ are negative have been closely examined and there would be no justification for redrawing the contour charts to produce positive values of $\zeta + f$, although this could easily be done. The smoothness of the $\zeta^{\times}$ field compared with the ζ^{+} field in the region of the jet stream shown in Fig. 3 (a) is typical, as are the rather small differences in regions of weak vorticity.

There is a field h' with the same boundary values as h and such that ζ^{+} as calculated from h' is the same as $\zeta^{\times}$ calculated from h. We computed the values of $\zeta^{\times}$, integrated the Poisson equation to give the field h' and formed the difference $h - h'$. Table 3 gives the difference $h - h'$ for the data of Table 1. Originally we intended to carry out the integration for all 18 cases but the first 10 integrations referred to those cases in which the differences $\zeta^{\times} - \zeta^{+}$ were most marked, so that $h - h'$ would be expected to be greatest. We found in these 10 cases that the maximum value of $h - h'$ was 50 ft and this was an isolated case (occurring in Table 3). At the 1880 grid points considered the difference $h - h'$ exceeded 20 ft on only 123 occasions and exceeded 40 ft on only 10 occasions, three of which were suspect. We therefore assumed that the differences arising between ζ^{+} and $\zeta^{\times}$ were no more than could arise from differences between two analyses of the 500-mb chart.

5. The results of the Jacobian calculations

The Jacobian is formed from first and third derivatives of the height field and is naturally rather rough; however, well-marked features in one Jacobian field were generally discernible in the other three fields, although the features were often displaced and of different emphasis. These marked features correspond to some marked feature in the height field such as a strong jet stream or strongly curved motion.

The maximum absolute value of the Jacobian was 181 ft hr^{-1} and values as large as 70 ft hr^{-1} were quite common. The differences between the computed values at a given point were often as great as the values themselves, but these large differences were often associated with large differences of the opposite sign in the immediate neighbourhood, there being often a relative displacement of the maxima and minima of the two Jacobian fields. Owing to this relative displacement it is difficult to associate even the sign of the Jacobian with some particular feature in the height field, e.g., it is not possible to say that the Jacobian has a particular sign in the rear of a depression, although on general grounds we should expect it to be negative in this region. In straight flow the Jacobian is usually rather small and of indeterminate sign suggesting that there is little change in these regions; the differences were also rather small and of indeterminate sign. There were occasional large Jacobians in flow of a rather indefinite character and these may have been occasioned by small inaccuracies in reading the 500-mb heights at some grid point.

Table 4 gives the values of J^{++} calculated for 15 GMT 30 Jan. 1952 and an examination of the differences between the other Jacobians and J^{++} showed that in eastern and southern parts of the chart there were fairly small values of the Jacobians and their difference, and in this region the vorticities and their differences were also small. In the north-west where there is a strong jet stream the differences in the Jacobians were quite marked, e.g., at the grid point 5 columns and 5 rows from the north-west corner the following values occur :

J^{++}	$J^{\times+} - J^{++}$	$J^{+\times} - J^{++}$	$J^{\times\times} - J^{++}$
181	− 246	− 70	− 215

Since the Jacobian has a high negative correlation with the height tendency it is clear that in the region there is a possibility of the different Jacobians leading to different signs of the calculated tendency. Table 5 gives the differences $J^{\times+} - J^{++}$ and the values given in Tables 4 and 5 are fairly representative of the other calculations made.

The $J^{\times\times}$ field is usually the smoothest, and J^{++} the roughest field; changing the method of calculating the Jacobian makes a greater difference than changing the method of calculating the Laplacian so that the fields J^{++}, $J^{+\times}$ were akin as were those of $J^{\times+}$ and $J^{\times\times}$. The mean of $J^{\times\times}$ and $J^{+\times}$ was found and proved on a whole to be smoother fields than those of any of the other Jacobians. Examination showed that the mean Jacobian fields could be associated with large-scale features on the synoptic chart and it may be better to use some mean Jacobian in the time integration.

We are able to reconstruct the height field corresponding to a given vorticity field and given boundary values; we are unable similarly to reconstruct the height field from the Jacobian field because the differential equation to be solved is not elliptic.

6. The time integrations

It has been shown above that changing the finite-difference scheme can make large differences to the calculated Jacobians. These differences may lead to forecast heights which are quite different; in the example which we have chosen to illustrate the differences it is evident that in some regions the sign of the height tendency will depend upon the finite-difference Jacobian used. In order to obtain the height tendency from the Jacobian field a double integration has to be performed and it is possible that this will have a smoothing effect which will remove the differences and lead to much the same values of $\frac{\partial h}{\partial t}$ whatever approximation is used; this is improbable owing to the large differences observed between the Jacobians. It was decided to investigate the effect of using the different vorticities and Jacobians in a 24-hr time integration. The barotropic model affords the simplest computation which involves the vorticities and Jacobians and which leads to results which agree in a fair measure with the development which takes place in the atmosphere. There would be no difficulty in carrying out a baroclinic forecast but it was felt that the extra complication would not help in deciding the effect of using the various finite-difference approximations. A programme was therefore written for the solution of

$$\nabla^2 \frac{\partial h}{\partial t} = -J\left(h, \frac{g}{f}\nabla^2 h + f\right) \quad . \quad . \quad . \quad (12)$$

in which one of four finite-difference analogues of the Jacobian can be inserted (at will) on the right-hand side. The height h was taken to be the height of the 500-mb surface, f being the Coriolis parameter. The forecasts were made on the usual grid used at Dunstable, the time steps being of one hour; at the boundary we assumed a knowledge of h at $t = 0$, 12 hr and 24 hr and $\frac{\partial h}{\partial t}$ was taken as a constant in each 12-hr period. Values of the Jacobians, tendencies and height fields could be printed out at intermediate times. We chose as the first case for study that which gave the largest differences in the Jacobian fields, the data being those for 15 GMT 30 Jan. Two integrations were first carried out using J^{++} and $J^{\times+}$ which showed the greatest differences at time $t = 0$; the differences

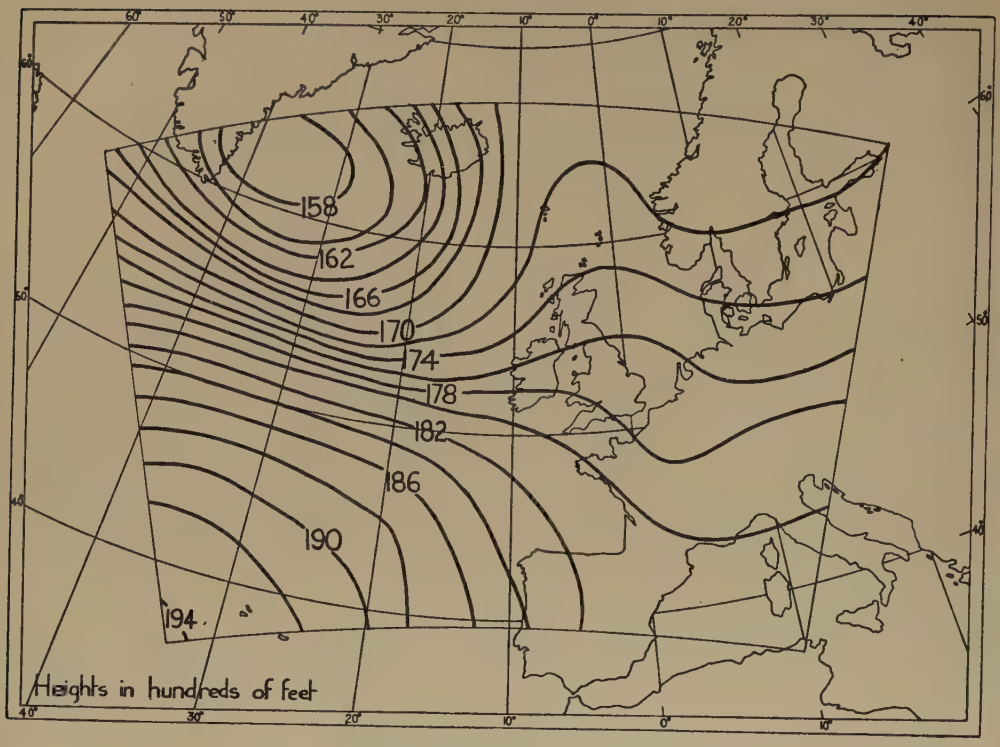

Figure 3 (a). 500-mb contours, 15 GMT 30 Jan. 1952.

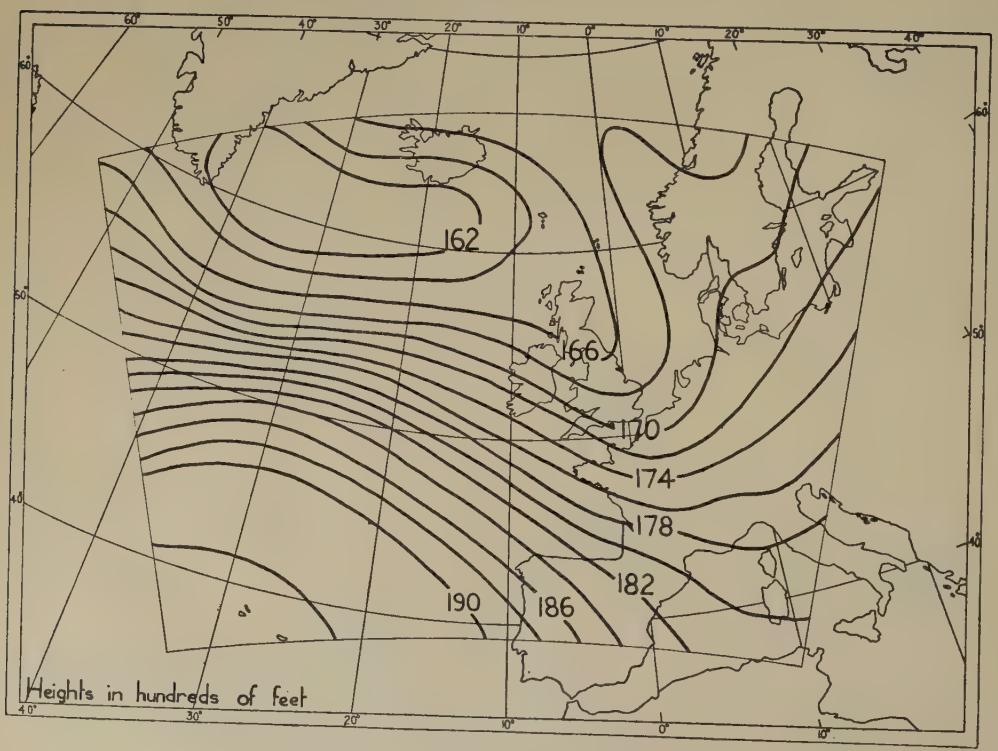

Figure 3 (b). 500-mb contours, 15 GMT 31 Jan. 1952.

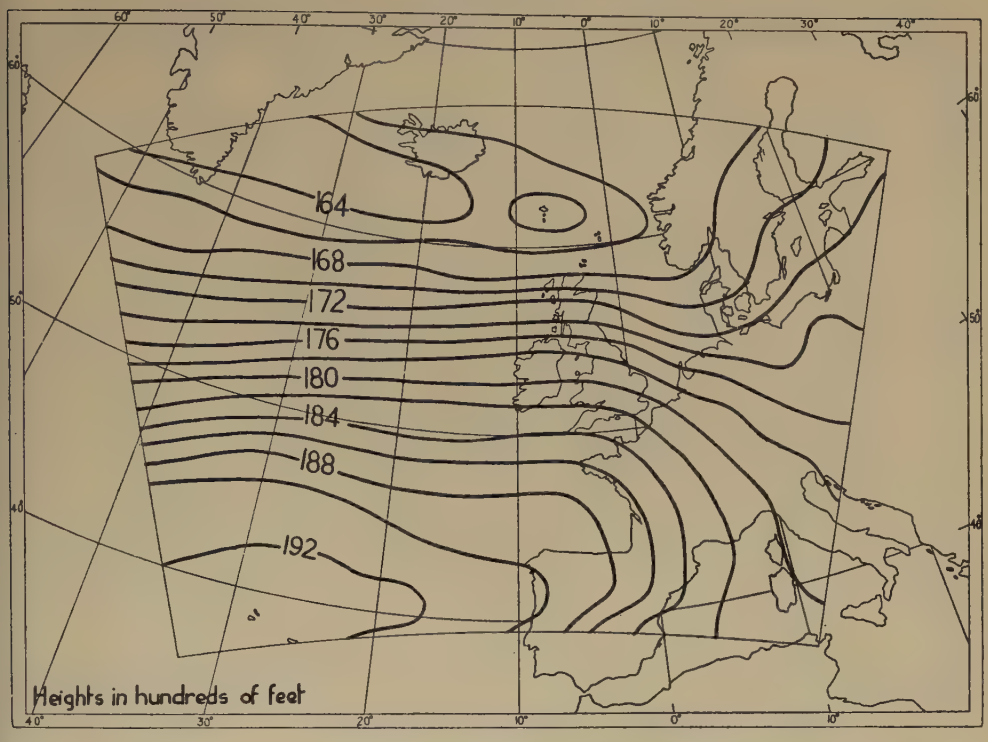

Figure 3 (c). Calculated 500-mb contours, 15 GMT 31 Jan. 1952 using J^{++}.

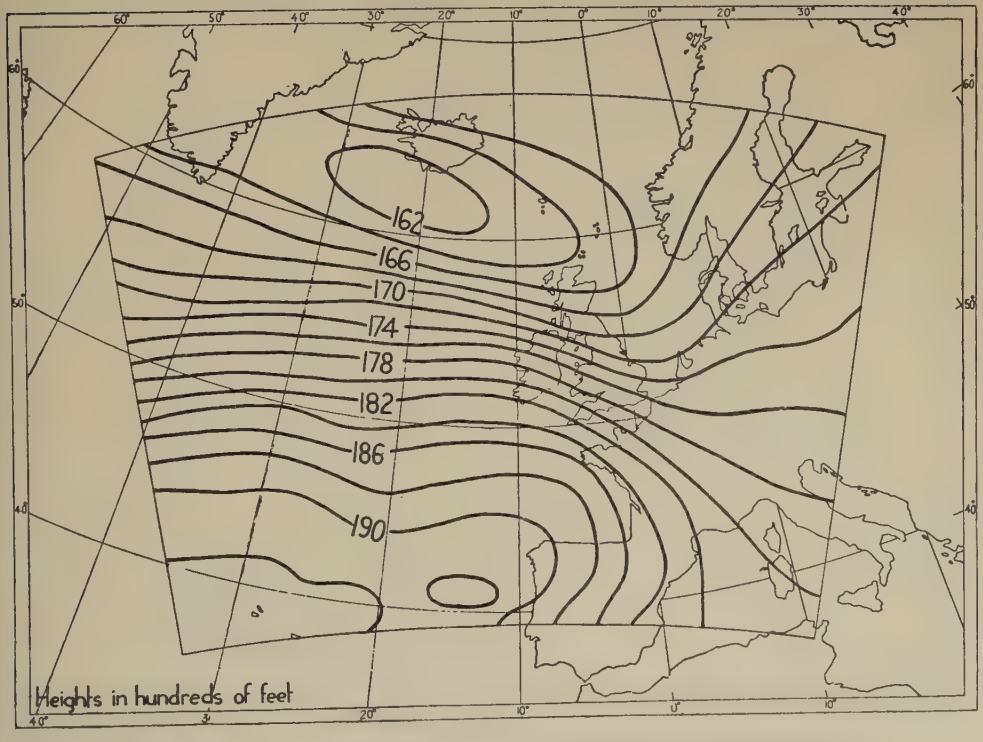

Figure 3 (d). Calculated 500-mb contours, 15 GMT 31 Jan. 1952 using J^{x+}.

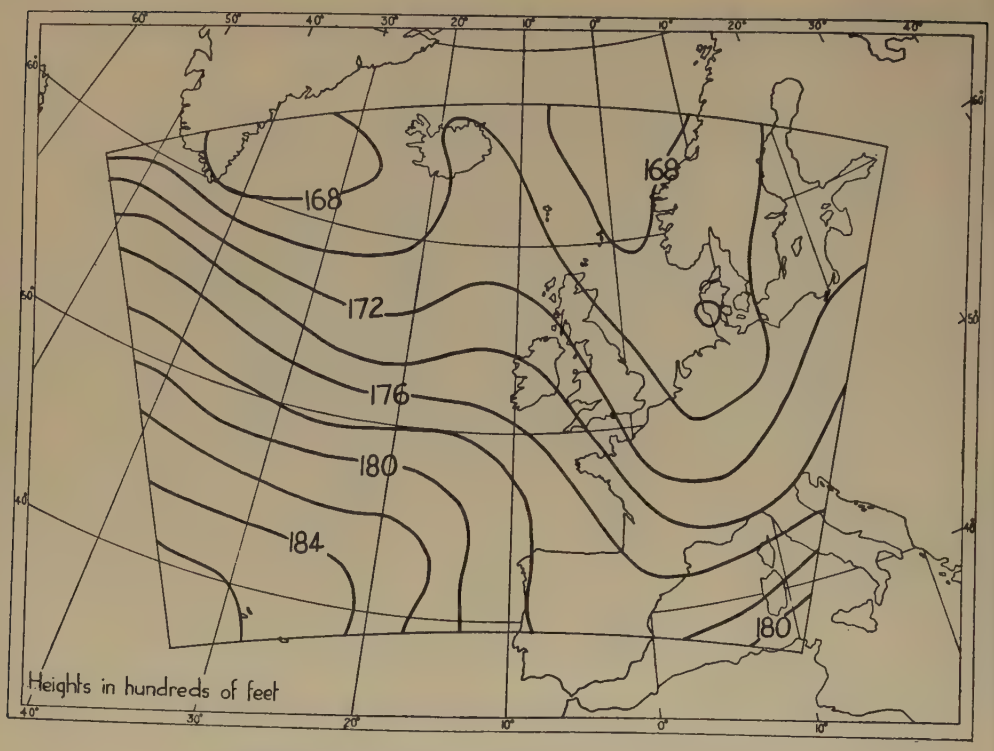

Figure 4 (a). 500-mb contours, 15 GMT 27 Jan. 1952.

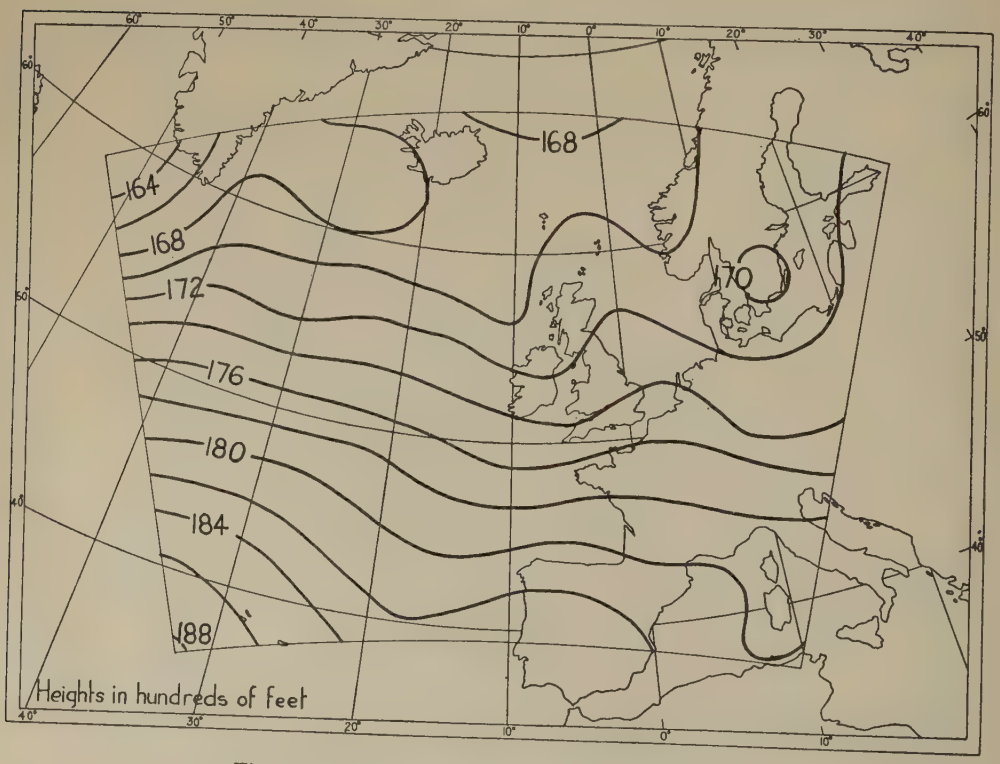

Figure 4 (b). 500-mb contours, 15 GMT 28 Jan. 1952.

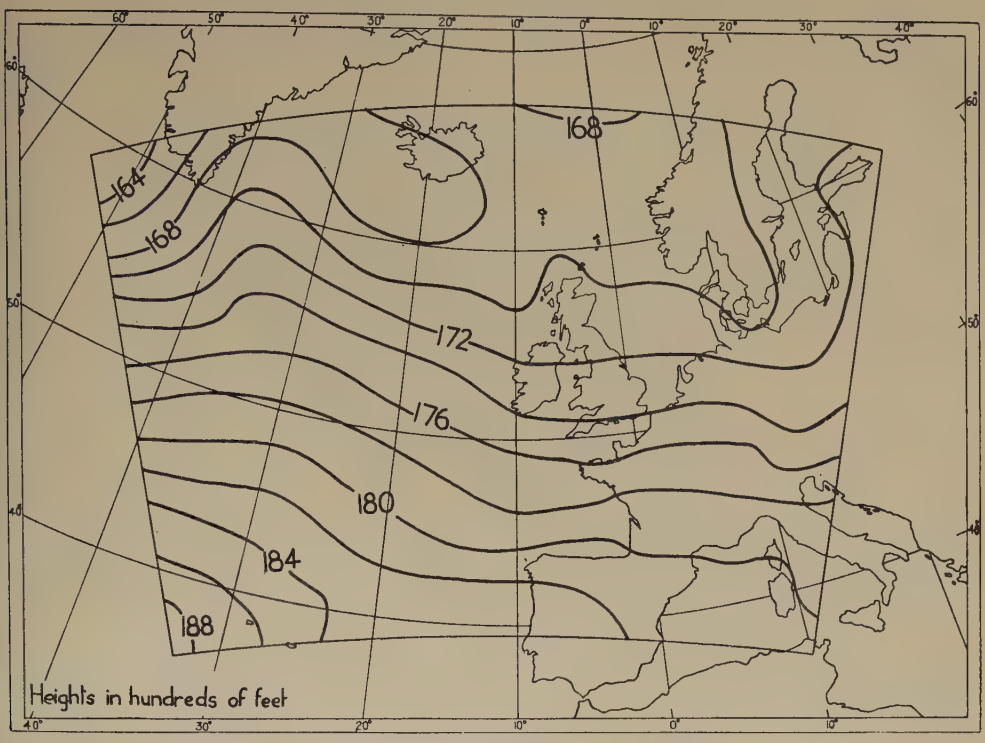

Figure 4 (c). Calculated 500-mb contours, 15 GMT 28 Jan. 1952 using J^{++}.

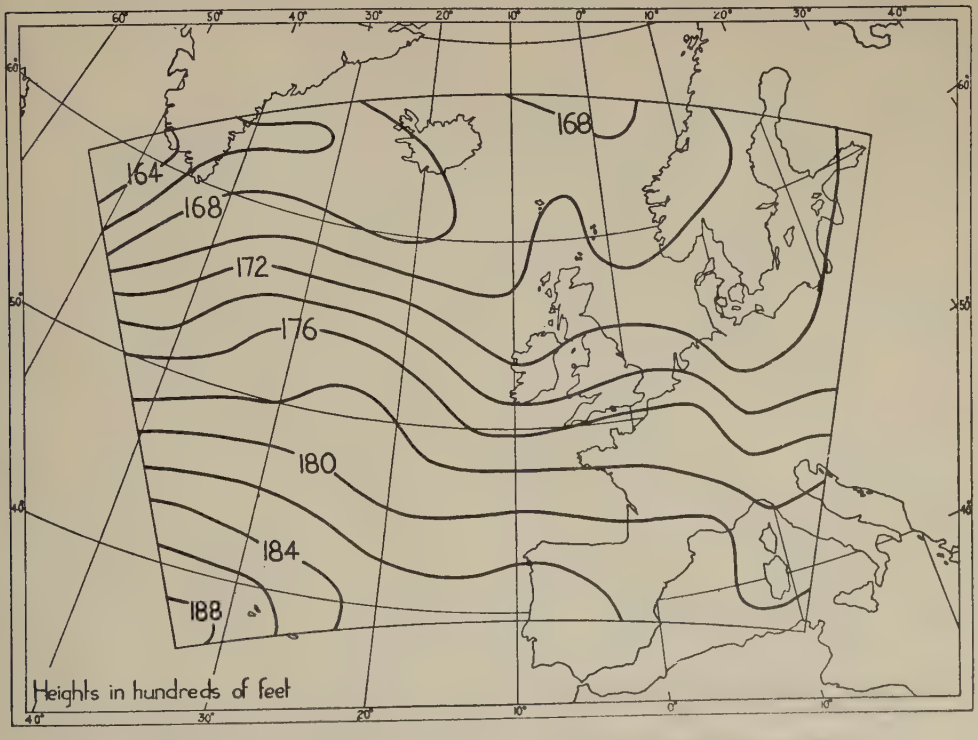

Figure 4 (d). Calculated 500-mb contours, 15 GMT 28 Jan. 1952 using $J^{\times+}$.

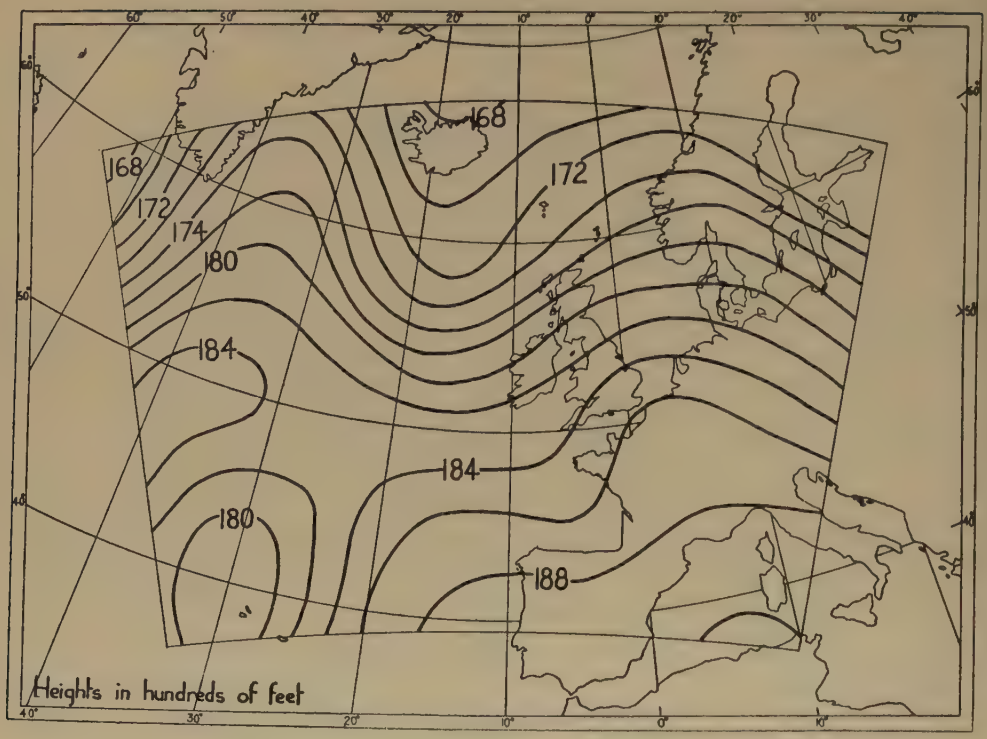

Figure 5 (a). 500-mb contours, 15 GMT 30 Jan. 1953.

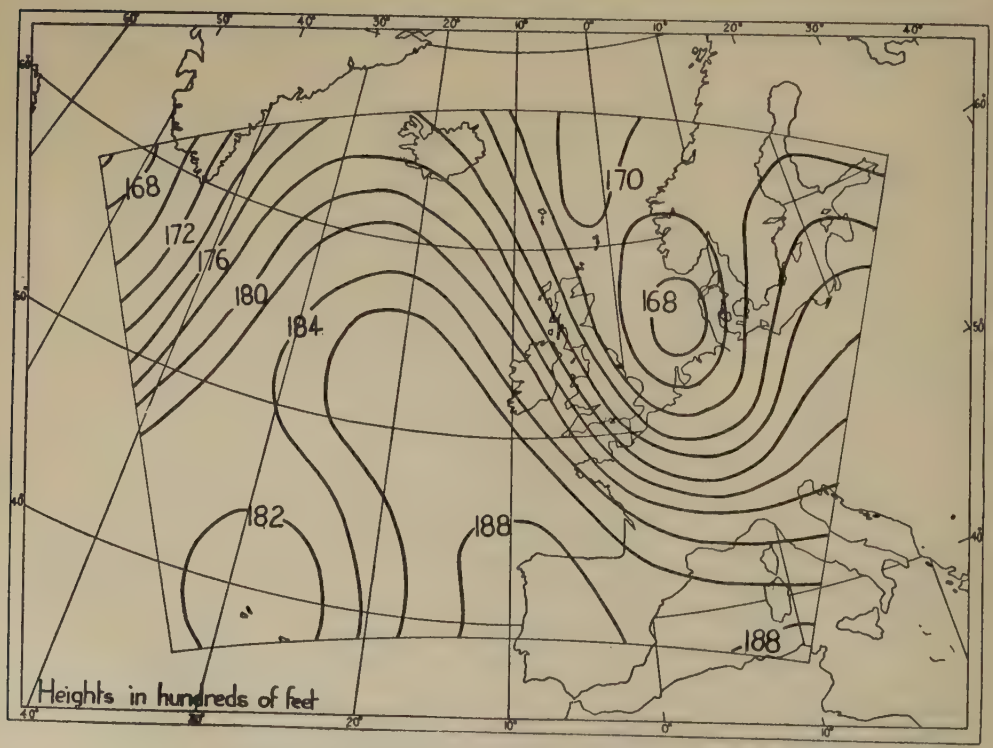

Figure 5 (b). 500-mb contours, 15 GMT 31 Jan. 1953.

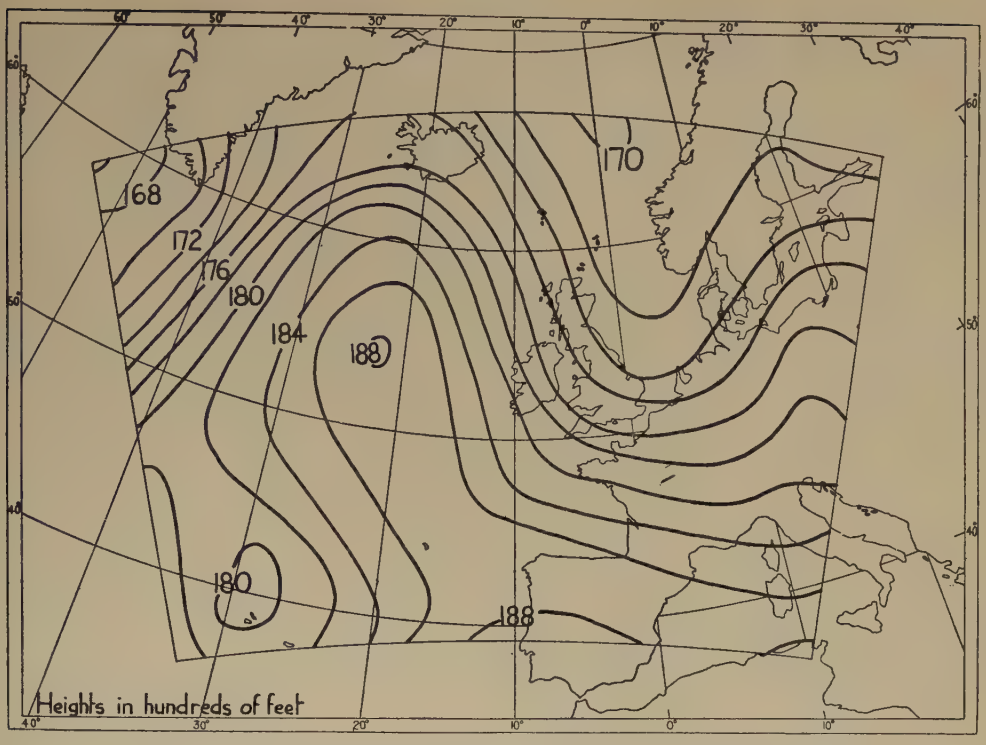

Figure 5 (c). Calculated 500-mb contours, 15 GMT 31 Jan. 1953 using J^{++}.

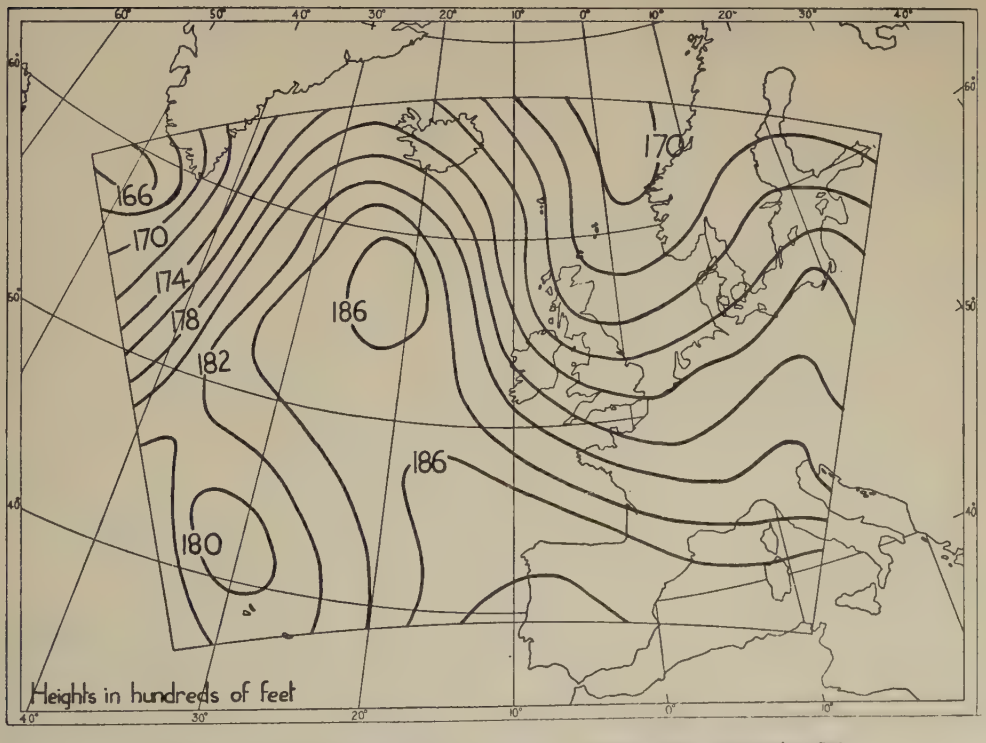

Figure 5 (d). Calculated 500-mb contours, 15 GMT 31 Jan. 1953 using $J^{\times+}$.

in the 24-hr forecast were sufficiently large to make it worth while to carry out the integrations using $J^{+\times}$ and $J^{\times\times}$ also.

The predicted height of the 500-mb surface at the inner 140 points using the four methods of calculating the Jacobian were compared; two of the corresponding contour charts are shown in Fig. 3 together with the initial height field and the actual final height field. The forecast made using J^{++} and that made using $J^{+\times}$ are almost identical; the greatest difference is 150 ft with 7 differences of over 100 ft and 103 of 50 ft or less. Similarly the forecasts using $J^{\times}$ are almost identical, with a greatest difference of 150 ft, 6 differences of 100 ft or more and 111 of 50 ft or less. On the other hand, the greatest difference between the forecast made using J^{++} and that made using $J^{\times+}$ is 720 ft, with 49 differences of 100 ft or more and only 51 of 50 ft or less. Table 6 gives the root mean square differences between the different forecasts. The large differences in the forecasts arise between those using a J^{+} and those using a $J^{\times}$ and we need consider only the differences between J^{++} and $J^{\times+}$, the differences between $J^{+\times}$ and $J^{\times\times}$ being similar, while those between J^{++} and $J^{+\times}$ are small.

The height field was printed out at each 3-hr interval, the vorticity, Jacobian and the tendency fields being printed at each 6-hr interval. The maximum differences between J^{++} and $J^{\times+}$, which were initially large, decreased, the largest difference falling from -246 at $t = 0$ to 155 at $t = 12$ hr and -157 at $t = 24$ hr. The r.m.s. differences over the 140 points considered, decreased at first but increased again, showing that the differences were increasing over most of the chart. The r.m.s. differences between J^{++} and $J^{\times+}$ were 45 ft hr^{-1} at $t = 0$, 33 ft hr^{-1} at $t = 12$ hr and 46 ft hr^{-1} at $t = 24$ hr.

The computed initial fields of $\frac{\partial h}{\partial t}$ using J^{++} and $J^{\times+}$ indicated that the double integration has considerably smoothed out the roughness of the original Jacobian fields and the values in the south are very much the same. The calculated rises in the rear of the depression are larger when J^{++} is used than when $J^{\times+}$ is used by the order of 10 ft hr^{-1} and the maximum rise is displaced further south by a grid length. The falls ahead of the low towards Scotland are also larger by about the same amount so that even at this stage it will not be surprising if the final forecasts differ considerably. The 6-hr forecasts of the height of the 500-mb surface are very similar, since the differences in $\frac{\partial h}{\partial t}$ can lead only to differences in height of 50-100 ft ahead of, and behind, the low; there is a slight but noticeable difference in the trough position.

The fields of $\frac{\partial h}{\partial t}$ after 6 hr showed that at this stage the difference in the isopleths in the upper central part of the field is quite marked, with the forecast using J^{++} moving the trough more rapidly eastwards with a greater surge of pressure behind. The 12-hr forecasts show the trough further east and north when J^{++} is used, the jet being almost west to east. The forecast made with $J^{\times+}$ holds the trough further back, extending it southward. At this stage we are effectively starting a 12-hr forecast with different initial charts using different methods of calculation and the divergence continues to grow.

The computed values of the tendency field after 24 hr are given in Table 7; that computed using J^{++} is out of phase with that computed using $J^{\times+}$, the centres having been moved further east and now appearing less intense. The differences between the fields have increased, the r.m.s. values being 11, 17 and 22 ft hr^{-1} at times $t = 0$, 12, 24 hr respectively, and the difference field is as great as the fields themselves. It is clear that further time integration would lead to more widely divergent forecasts.

Fig. 3 shows that the forecast made using $J^{\times+}$ was better than that made using J^{++}, the maximum errors being 970 ft and 1,290 ft respectively. The differences between the forecast values are not quite as great as the difference between the better forecast values and the actual final values, but are of the same order in the vicinity of the trough; near the boundaries in the north-west both forecast values are similar and differ from the actual values by several hundred feet.

In the region of the trough the differences between the forecast values are large; the maximum difference is 730 ft and at 8 grid points the difference exceeds 400 ft. These large differences occur because the forecast made using J^{++} moved the trough too far east without sufficient deepening and failed to predict the south-eastward extension. The forecast made using $J^{\times+}$ predicted part of the south-eastward extension and moved the trough more slowly in accordance with the actual development, but the considerable deepening in the region of the British Isles was not forecast. The increasing difference between the two forecasts is borne out by the r.m.s. differences between them, which are 103 ft after 12 hr and 177 ft after 24 hr.

In order to compare the two forecasts with one another and also with forecasts made from the same initial data using other methods of computation, the actual and forecast height changes have been correlated over the inner grid of 12 × 8 points, and the resulting correlation coefficients are given in Table 8. There is marked difference in the correlation coefficients indicating that the forecast made using $J^{\times+}$ was superior to that made using J^{++} and this is borne out synoptically by the charts shown in Fig. 3. The improvement made by using $J^{\times+}$ instead of J^{++} is as marked as is that made by any of the changes made in the method of use of the Sawyer-Bushby model.

7. Some further results

The synoptic situation of 30 Jan. 1952 is characterized by a strong jet stream; the existence of a jet is common although perhaps that occurring in this situation is stronger than usual. Similar experiments with two quite different synoptic situations and developments were carried out.

The initial chart used in the first of these refers to 15 GMT 27 Jan. 1952 and is given with the chart 24 hr later at 15 GMT 28 Jan. 1952 in Figs. 4 (a) and (b). Initially there was a low to the west of Iceland with its attendant trough stretching SSE. The ridge ahead of it was to the west of Ireland with a deep trough extending down the North Sea, through the Low Countries and curving backward into Spain. In the next 24 hr the trough extending from the Iceland low developed and lay down the Irish Sea, while the deep trough over the North Sea moved eastward and relaxed. The ridge between these two troughs moved off the eastern coasts of Gt. Britain and also relaxed so that the amplitude of the pattern was reduced. The 24-hr changes in the height of the 500-mb surface were generally less than those which occurred in the 24 hr following 15 GMT 31 Jan. 1952.

The 24-hr forecasts made with the use of J^{++} and $J^{\times+}$ are given in Figs. 4 (c) and (d). The 24-hr changes in height of the 500-mb surface over, and to the west of Gt. Britain were not more than about 300 ft so that minor differences in the forecast changes could lead to large differences in the positions of the relaxing trough and ridge; the pattern of height changes, a slight fall to the west of Gt. Britain and a rise to the east was well forecast by both the numerical methods. Table 9 which gives a statistical summary of the three time integrations, shows the high correlation between observed and forecast changes. The difference between the forecast height changes increased with time and was in the mean about 40 per cent of the actual change. Thus, although there appears to be little difference in the success of the two methods of forecasting as judged by the correlation coefficients, the forecast height changes at many points on the chart are different in magnitude and the troughs and ridge are relatively displaced.

The last synoptic situation chosen for examination refers to 15 GMT 30 Jan. 1953. On the night of 31 Jan. 1953 strong NW winds in the North Sea caused disastrous flooding on the east coast of Gt. Britain and on the coasts of Holland and Denmark. Numerical forecasts had already been made using the data of 15 GMT 30 Jan. 1953 and we chose to make two barotropic forecasts from the same data. At this time a deep trough with strong NW winds stretched almost due south from Iceland with a ridge ahead over the North Sea, Gt. Britain being in a SW airstream of about 50 kt. In 24 hr the trough developed rapidly, and advanced with a closed centre over the southern North Sea and very strong

N and NW winds over Gt. Britain, reaching 119 kt at Larkhill, while a strong ridge almost replaced the trough.

The synoptic situations at 15 GMT 30 Jan. 1953 and 15 GMT 31 Jan. 1953 are shown in Figs. 5 (a) and (b), the numerical forecasts using J^{++} and $J^{\times+}$ being given in Figs. 5 (c) and (d). The pattern of changes, a rapid fall in the region of Gt. Britain and the North Sea with a rapid rise to the west, was well forecast, giving a high correlation coefficient between the forecast and actual changes in each case. As in Table 9, however, the resulting forecast charts showed the trough too far west and under-emphasized both the depth and the very strong northerly winds which were the most important feature of the synoptic situation. The r.m.s. of the difference between the forecast height changes again increased almost linearly with time, but were only about 10 per cent of the actual changes after 24 hr. The forecast made using J^{++} was, from a synoptic point of view, more successful than that made using $J^{\times+}$ since it moved the trough further east and gave a lower pressure within it; neither forecast gave the closed low that actually existed after 24 hr.

8. Conclusion

In the experiment described above, two sets of forecasts have been made from the same data using the same method, the only difference introduced being in the orientation and length of the mesh. The 24-hr forecast on two of the three occasions differed considerably in an area near the centre of the mesh which is usually supposed to be the most free from errors which arise because of the boundary conditions. The difference between the two forecast values of the changes in the height field of the 500-mb surface increased with time and was about 40 per cent of the actual change in two cases and about 10 per cent in the third. The implication is grave, that upon some occasions the choice of mesh size and orientation in the field has a decisive effect upon the forecast that is made. There can be little doubt that the differences depend very largely upon the computed Jacobians and the remedy may lie in a more accurate determination of derivatives by taking into account values at more grid points in computing them.

Acknowledgment

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Reference

Sawyer, J. S. and Bushby, F. H. 1953 *J. Met.*, **10**, p. 54.

TABLE 1. Relative vorticity ζ^+ 15 GMT 30 Jan. 1952 (10^{-2} hr^{-1}) calculated from grid points 0, 1, 2, 3, 4 of fig. 1

−021	017	008	062	017	039	005	−032	−013	008	004	001	008	016	006	004
−037	045	043	056	044	029	051	−004	−014	−018	001	008	011	006	008	001
004	−012	046	064	049	049	049	009	−021	−023	−007	015	010	−007	001	014
−019	002	001	026	040	028	029	−007	−010	−014	−022	010	001	005	006	028
−042	−044	001	022	047	047	027	020	017	011	−013	−002	002	023	015	000
−012	−036	−047	−047	−044	015	032	027	−002	−014	−007	−008	006	006	−006	−002
000	−004	−009	−019	−026	−031	−030	−026	−015	−006	001	010	004	−005	−008	003
−034	−011	−015	−017	−020	−019	−027	−005	−003	−016	009	019	004	−008	000	−006
−013	−014	−007	−004	−008	−008	−022	−004	001	−003	−003	010	007	000	000	002
002	−018	−016	−016	−009	−015	−012	−006	−007	009	−001	−006	002	000	003	−003
−015	−002	−009	−008	−010	−018	−011	−010	006	002	002	003	008	−002	002	−004
−008	−010	−006	−011	−020	007	−004	−010	−005	019	007	014	002	−002	004	000

TABLE 2. RELATIVE VORTICITY DIFFERENCES $\zeta^{\times} - \zeta^{+}$ 30 JAN. 1952 (10^{-2} hr^{-1})

+	000	008	−015	009	−012	004	010	−001	−006	000	001	002	−004	001	001
018	−015	−003	003	−006	009	−011	002	001	006	−002	−000	−001	001	−002	004
−014	020	−010	−004	002	−003	−001	−005	006	005	−001	−003	−002	005	001	−003
005	−010	009	001	−006	002	−002	004	−002	−000	005	−004	002	001	002	−008
005	011	−004	001	−009	−003	007	−001	−004	−005	005	004	003	−006	−005	007
−005	−001	006	005	014	−006	−009	−008	003	005	−003	005	−003	−001	002	001
−007	−003	−002	001	−002	007	002	005	−001	−005	−001	−002	000	002	003	−003
007	−006	−001	000	000	001	004	−003	−003	006	−004	−005	001	003	−003	003
001	002	−001	−002	001	−001	004	000	−002	001	003	−001	−001	−001	−001	−003
−009	004	001	002	−002	002	−001	−001	−004	006	000	004	000	−002	−001	003
006	−005	002	−001	−002	005	000	002	−006	004	000	001	−002	001	000	000
000	001	000	−001	005	−008	004	001	003	−008	006	−005	003	001	−001	+

TABLE 3. HEIGHT DIFFERENCES IN FEET CALCULATED FROM RELAXATION OF TABLE 2, 15 GMT 30 JAN. 1952

+	004	−001	050	003	037	−002	−036	−011	007	−002	−004	−003	008	−001	−006
−034	030	029	027	031	009	034	−011	−024	−030	−004	002	003	−002	003	−007
012	−030	029	035	026	028	024	007	−030	−034	−010	008	004	−014	−001	009
−012	001	−012	013	036	022	018	−002	−002	−012	−022	000	−007	−002	002	019
−020	−033	−010	−002	026	023	004	012	014	007	−019	−020	−009	015	011	−008
006	−010	−027	−031	−033	013	029	028	005	−004	002	−012	004	005	−004	−004
014	007	−004	−016	−017	−021	−007	−002	008	015	012	010	002	−006	−007	001
−007	010	001	−006	−012	−018	−018	001	008	−001	016	017	002	−007	000	−004
−001	−002	000	000	−007	−010	−017	−002	007	006	003	007	003	000	−002	002
011	−004	−004	−004	−002	−011	−006	−002	−001	012	002	−006	001	003	000	−005
−004	005	−002	−001	−001	−010	−006	−003	009	001	−001	−001	003	−001	000	−001
−001	−001	−001	−001	−006	006	−006	−003	−001	010	−005	005	−002	−002	−000	+

TABLE 4. COMPUTED JACOBIAN J^{++} (ft hr^{-1}) 15 GMT 30 JAN. 1952

041	071	001	−011	−010	−067	−060	010	018	011	006	−001	004	001
005	083	−031	−021	004	016	−018	−014	006	019	009	−013	−004	009
001	−015	056	006	−021	−024	−049	−046	−024	014	011	−004	−001	008
064	126	046	000	−060	−060	−046	−013	−019	−009	006	017	008	−013
−039	−037	−039	116	181	027	−053	−044	−001	005	009	009	−004	002
000	−003	−010	−009	−034	−016	018	026	013	013	005	−009	−004	003
016	−006	003	008	003	024	041	−003	008	018	−004	−015	−003	−001
001	006	−001	002	−010	013	014	−005	011	000	−001	−005	−004	000
−004	000	000	−002	−010	010	−006	008	003	−005	−001	002	001	−001
004	002	001	−011	014	006	−003	002	003	003	004	−002	−002	−001

TABLE 5. THE DIFFERENCE $J^{\times +} - J^{++}$ (ft hr^{-1}) 15 GMT 30 JAN. 1952

051	−043	027	008	−028	040	038	−013	−009	−006	−003	−001	−006	004
076	−114	042	001	006	−034	011	011	000	−006	−004	011	006	−010
038	151	−018	−004	006	−028	−001	009	013	−006	−004	005	002	−011
−106	−189	−066	064	147	031	−016	−026	009	019	007	−015	−010	023
089	099	033	−151	−246	−050	074	066	−004	−005	−003	−006	006	−003
−009	010	033	071	099	031	−031	−063	−009	004	−003	004	−001	−002
−020	004	−008	−007	−008	−005	−014	018	−002	−010	006	008	−003	003
006	−009	003	−002	013	−004	−003	012	−012	004	−001	000	004	−001
006	002	−003	−008	008	−015	018	−008	−003	006	004	−005	−003	002
−006	000	−003	020	−016	−002	004	003	000	000	−004	000	003	001

TABLE 6. THE R.M.S. DIFFERENCE BETWEEN 24-HOUR FORECASTS USING DIFFERENT JACOBIANS

Forecasts using	$J^{\times+}$, J^{++}	$J^{+\times}$, J^{++}	$J^{\times+}$, $J^{\times\times}$
R.M.S. difference (ft)	177	54	47

TABLE 7 (a). VALUES OF $\frac{\partial h}{\partial t}$ COMPUTED AT 15 GMT 31 JAN. 1952 USING J^{++} (ft hr^{-1})

−001	001	016	019	019	026	022	001	−012	−019	−020	−016	−009	−008
−004	−001	012	004	011	025	022	009	−008	−019	−029	−031	−018	−010
−010	−006	−006	002	007	009	019	009	011	019	−009	−040	−035	−011
−024	−039	−043	−003	008	−023	−026	004	014	029	029	−005	−031	−012
−009	−014	−036	−004	016	−012	−043	−021	−003	031	031	011	−011	−006
007	−003	−011	015	016	−007	−018	−014	−023	009	023	020	004	006
004	006	011	022	014	−002	−009	−016	−014	015	021	024	017	013
−006	−003	008	013	010	006	−006	−009	005	021	021	038	032	014
−012	−005	004	007	015	010	001	006	015	024	029	034	019	008
−013	−009	−001	004	011	012	015	023	034	056	060	036	018	010

TABLE 7 (b). VALUES OF $\frac{\partial h}{\partial t}$ COMPUTED AT 15 GMT 31 JAN. 1952 USING $J^{\times+}$ (ft hr^{-1})

010	009	009	016	022	014	−002	−012	−016	−018	−018	−014	−008	−002
−001	012	011	021	019	009	−002	−011	−020	−031	−031	−024	−017	−008
−014	−012	−003	016	009	005	001	001	−008	−033	−043	−036	−020	−012
−031	−010	000	−006	009	−008	−008	006	015	−011	−046	−042	−018	−014
−029	−032	−001	018	−032	−030	000	018	038	004	−031	−031	−018	−011
−023	−011	016	−003	−028	−048	−025	014	039	041	−002	−014	−011	−009
−009	−009	014	018	−028	−048	−021	004	036	038	026	011	−003	−004
−009	−003	012	009	−015	−029	−014	012	034	048	041	021	006	−001
−011	−006	−001	003	−008	−011	004	022	033	041	033	028	011	002
−013	−009	−003	000	002	006	020	038	051	051	043	030	016	004

TABLE 8. CORRELATION OF FORECAST HEIGHT CHANGE WITH ACTUAL HEIGHT CHANGE

Method of forecast		Correlation coefficients
(a) Barotropic using J^{++}		+ 0·48
(b) Barotropic using $J^{\times+}$		+ 0·72
(c) Sawyer-Bushby model	500 mb, zero boundary changes	+ 0·61
(d) Sawyer-Bushby model	600 mb, zero boundary changes	+ 0·59
(e) Sawyer-Bushby model	500 mb, zero boundary changes, heating effect	+ 0·62
(f) Sawyer-Bushby model	600 mb, actual boundary changes, heating effect	+ 0·47
(g) Stream function	600 mb, actual boundary changes, heating effect	+ 0·76

TABLE 9. STATISTICAL SUMMARY

		Forecast commencing at 15 GMT		
		30 Jan. 1952	27 Jan. 1952	30 Jan. 1953
Correlation of forecast and actual 24-hr height changes at 500 mb	J^{++}	0·48	0·94	0·95
	$J^{\times+}$	0·72	0·91	0·92
Using Sawyer-Bushby model (time integration at 600 mb)		0·47	0·93	0·96
R.M.S. of forecast height differences using J^{++} and $J^{\times+}$	12 hr	103 ft	37 ft	59 ft
	24 hr	177 ft	73 ft	96 ft
R.M.S. actual change	24 hr	401 ft	256 ft	624 ft
Ratio $\frac{\text{R.M.S. (difference between 24-hr forecasts)}}{\text{R.M.S. (actual 24-hr change)}}$		0·44	0·39	0·12
$\frac{\sigma \text{ (forecast 24-hr change using } J^{++})}{\sigma \text{ (actual 24-hr change)}}$		1·08	0·91	0·89
$\frac{\sigma \text{ (forecast 24-hr change using } J^{\times+})}{\sigma \text{ (actual 24-hr change)}}$		1·23	0·96	0·82

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The determination of tropospheric ozone from infra-red emission spectra

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SUMMARY

The emission spectrum of the sky has been measured on 127 occasions at Ascot in a period of 12 months in 1956 and 1957, and the observations have been interpreted in terms of the total amount of ozone between 1,000 and 300 mb. The ozone amount in this layer varies about a mean value of 13×10^{-3} cm STP and an individual determination is accurate to about $\pm 3 \times 10^{-3}$ cm STP.

The results suggest that fluctuations in the total ozone amount are controlled by different mechanisms in spring and the rest of the year, and that during this period advection may be the dominating effect. There is evidence that some of the tropospheric ozone has come from the upper stratosphere, but it also seems probable that much has been advected into these latitudes.

Our results show very little relation to ground-level chemical measurements made just after mid-day. This disagreement might possibly be due to secondary sources of ozone near ground-level.

1. INTRODUCTION

The work described in this paper is part of an extensive programme relating to the absorption and emission of radiation in the atmospheric window near 1,000 cm^{-1}. When the results were first examined it became clear that the 1,045 cm^{-1} band of ozone was a special case because although most of the ozone lies above 15 km, most of the emitted radiation originates from below this level, so that the observations contain information on the vertical distribution of this gas. Since at that time (1955) other methods of measurement for layers below 15 km were cumbersome, expensive, or inaccurate, this suggested that observations could lead to new information on the ozone problem, and a year's series was undertaken, the results of which are presented in this paper.

Early measurements were made in London, but the intensity of the background absorption and emission cast doubt upon the results, so the series was not started until February 1956, when the spectrographic equipment was removed to the Imperial College Field Station at Ascot, approximately 40 km west of London, at an altitude of 66 m above sea level.

During the year 127 reliable sets of observations have been made, but despite this good record it has not been thought worth while to extend the series since the bad climate places us at a very great disadvantage compared with other observers. The haze correction is much larger than is desirable, and in this country it is probably as bad as it can be, and our results also probably suffer from our inability to take simultaneous absorption and emission spectra.

Similar work to that described here has been performed at Flagstaff, Arizona by Epstein, Osterberg and Adel (1956). The two efforts are independent although the Flagstaff work must have started about a year before ours. The two methods are identical in matters of principle but a number of very important differences in detail existed at first. A conference was held in London in September 1956, under the auspices of the International Ozone Commission, to recommend techniques of observation and analysis for use in routine observations and these recommendations (referred to as IOC*) are followed in this paper.

* Copies of the papers of this conference may be obtained from Sir Charles Normand, 56 The Holywell, Oxford.

It is worth reviewing in general terms the principles used in this work. Consider four atmospheric layers between the pressure levels shown in Table 2. The ozone amount in layer 4 is assumed to be controlled by photochemical processes and is therefore known. The total ozone amount, as measured by a Dobson spectrophotometer, is therefore a function of the ozone amounts in layers 1, 2 and 3 and principally of the amounts in layers 2 and 3 on account of the maximum of concentration in the stratosphere. The band area in absorption, which we do not measure but infer from the observations of Walshaw and Goody (1956), is, on account of the powerful effect of pressure broadening, mainly a function of the ozone amounts in layers 1 and 2. The emission measurements are not only influenced by pressure broadening but also by the temperature, through the sensitive Planck black-body function. This gives an additional weighting towards layer 1 so that the observations depend strongly on the amount in this layer.

The three observables, total amount and band areas in absorption and emission are therefore sufficiently independent for the amounts in the different layers to be computed. The method of computation has been described in an earlier paper by Goody and Roach (1956, referred to as GR), and in the next section we will only add to and not repeat what has already been said on this subject.

The symbols used in this paper have already been defined in GR except that here ϵ is used in place of ρ for the mixing ratio in order to avoid confusion with density.

$\tau(b)$ = transmission of the background near 1045 cm^{-1}

$I(b)$ = emission of the background (erg cm^{-2} sec^{-1} $ster^{-1}$ (wave number)$^{-1}$)

B = Planck function at 1,045 cm^{-1} (erg cm^{-2} sec^{-1} $ster^{-1}$ (wave number)$^{-1}$)

$\bar{B}_1, \bar{B}_2, \bar{B}_3$ = mean Planck functions for layers 1, 2 and 3

ϵ = ozone mixing ratio (cm STP mb^{-1})

p = pressure (mb)

$\bar{p}$ = spectrographic mean pressure (mb)

u = average tropospheric southward wind velocity (knots)

m_1 = ozone amount 1,000 to 300 mb (cm STP)

m = total ozone amount (cm STP)

m_0 = ground-level ozone (cm STP)

δ = deviation from running mean

2. Points of technique

(a) *Corrections*

The analysis as presented in GR assumes there is no emission from a liquid-nitrogen surface, but this is not quite correct for there is detectable reflection from the surface. This affects the analysis in a number of ways, but it is found that if the liquid nitrogen is consistently assumed to have no emission, the first-order errors cancel, and the resultant error can be neglected. No correction was therefore applied for this effect.

The background absorption and emission (probably owing to haze) have been the subject of further work from which it appears that the effective temperature is that of the 950-mb level. This enables the transmission of the background to be determined separately for each individual observation from the relation

$$\tau(b) = 1 - I(b)/B(950)$$

Two weak infra-red bands of carbon dioxide are to be found at 1064 and 961 cm^{-1}. The positions of these bands relative to the ozone band are shown in a high-resolution spectrum published by Goody (1957). It is not possible to decide objectively what effect these bands have upon the emission areas of the ozone band since the backgrounds are not fitted in an objective manner. After careful consideration, however, it was decided that the 961 cm^{-1} band would not influence the results and that about half of the 1,064 cm^{-1} band would be erroneously included in the ozone band. We were supplied by Dr. L. D. Kaplan with complete details of the 1,064 cm^{-1} band and were able to compute its curve of growth and effective emission temperature. It was found that for a normal temperature distribution the emission temperature was the same as the temperature at 710 mb, so the correction, to be subtracted from the observed band area, has been tabulated in terms of the temperature at this level and is shown in Table 1 for air masses 2 and 5.

TABLE 1. CORRECTIONS FOR CARBON-DIOXIDE EMISSION; TO BE SUBTRACTED FROM MEASURED AREAS (erg cm^{-2} sec^{-1} $ster^{-1}$)

T (710) °K		280	275	270	265	260	255
air mass	2	122	101	84	69	56	44
	5	220	185	153	126	104	83

(*b*) *Analysis*

The analysis is performed in terms of discrete layers with assumed distributions of matter inside each layer. The adopted conventions are shown in Table 2, and these accord with the IOC recommendations.

TABLE 2. CONVENTIONS FOR LAYERS

Layer	p_1 (mb)	p_2 (mb)	ϵ (cm STP mb^{-1})	$\int_{p_2}^{p_1} \epsilon\, dp$	$\int_{p_2}^{p_1} \epsilon p\, dp$
1a	1000	600	ap^{-1}	0·508 a	400 a
1b	600	300	ap^{-1}	0·694 a	300 a
2	300	50	bp^{-2}	0·0167 b	1·790 b
3	50	10	$cp^{-0·2}$	20·70 c	600·5 c
4	10	0	summer	45×10^{-3}	0·255
			winter	27×10^{-3}	0·152

The lowest layer is split in two, without introducing further independent variables, to allow mean Planck functions to be used as defined by GR Eq. 6. This simplification is permissible if the layer is optically thin or if the temperature does not vary too much inside the layer. In both respects this is least correct for layer 1, but by dividing into two the approximation becomes acceptable for the two halves separately.

The errors involved in treating each layer as an entity were tested in the following way. Imaginary data were put into model atmospheres with 22 independent layers. The emission was then computed as a function of air mass, and from these data the ozone amounts in the layers defined by Table 2 were recovered by the methods used in this paper. In the examples tried, the recovered amounts corresponded closely with the original data with residuals much smaller than those caused by other sources of error.

For the purpose of evaluating mean Planck functions, layer 1a was divided into four, layer 1b into three and layer 2 into six, and computations were made from the upper-air data for each day of observation. For layer 4 the IOC conventions are based on the

work of Murgatroyd (1957) which for lat. 50° gives 40·5 and 17·0 ergs cm^{-2} sec^{-1} $ster^{-1}$ (wave number)$^{-1}$ for summer and winter half-years respectively.

Normal radiosonde ascents in this country only reach to the bottom of layer 3, but the Meteorological Office kindly made available 18 ascents to pressures below 10 mb, distributed fairly evenly through the year. A feature of these ascents was that the correlation between the temperatures at various levels in layer 3 was very good. The mean gradient in the layer was closely correlated with the temperature at the bottom of the layer in the sense that the higher the temperature the more positive the temperature gradient. This leads to the great advantage that the mean Planck function for layer 3 can, within ± 3 per cent, be given in terms of the mean temperature between 40 and 50 mb, which is available from the daily upper-air charts. This empirical function is shown in Table 3, and was used in the analysis of results.

TABLE 3. MEAN PLANCK FUNCTIONS FOR LAYER 3 (ergs cm^{-2} sec^{-1} $ster^{-1}$ (wave numbers)$^{-1}$)

T (40-50) °K	200	205	210	215	220	225	230
B_3	7·0	8·4	10·2	12·3	16·0	18·9	21·4

It was stated in GR that the largest errors in the analysis arise from uncertainties in the upper-air temperatures. With this new treatment of layer 3 this is no longer the case and the accuracy is now controlled by the errors of observation and the errors involved in assuming a value for the spectrographic mean pressure (see below).

In GR we expressed doubts as to the usefulness of measuring the absorption area of the band, or what comes to the same thing, the spectrographic mean pressure $\bar{p}$. This may not be correct, and our results would probably have been improved if simultaneous solar measurements had been made. This would, however, have resulted in a very small number of observations, for conditions suitable for absorption spectra and for a zenith scan in emission are very rare in this country. As a result we decided to concentrate entirely on getting a good series of emission spectra, usually during the night time, and as in our previous paper we had to assume mean monthly values of $\bar{p}$ as shown in Table 4. The values shown in this table are about 10 mb higher than the average of all the results of Walshaw and Goody (1956), but they are well within the range of their measurements; this is because, particularly in winter, the true average $\bar{p}$ was on occasion a little too low to provide a solution.

TABLE 4. $\bar{p}$ (mb) AFTER WALSHAW AND GOODY (1956).

Month	Jan.	Feb.	March	April	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.
$\bar{p}$	60	60	70	80	80	90	90	80	70	60	60	60

As far as layer 1 is concerned the errors involved in assuming a mean pressure are small. A number of trial computations has been made and we find that the error given in GR is if anything a slight over-estimate. The estimate of observational error made in GR remains correct, so the error from all sources of the results discussed in this paper is about $\pm 3 \times 10^{-3}$ cm STP.

For the other layers errors in $\bar{p}$ are of critical importance and we doubt whether our results for these layers are of any significance apart from the fact that on the average the remaining ozone is distributed fairly evenly between layers 2 and 3. As implied in the title, therefore, this paper will concern itself only with results from the lowest, tropospheric, layer.

It is unfortunate that the network of Dobson spectrophotometers has, up to the present, made use of the ultra-violet absorption coefficients of Ny and Choong, since these are undoubtedly incorrect (Hearn, Walshaw and Wormell 1957). To be consistent with this practice Walshaw's original reduction of his laboratory data on the 1,045 cm^{-1} band also used Ny and Choong's coefficients, and this reduction was the only one available to us when analysis of our results was started. Since then Walshaw (1957) has presented his data in terms of more acceptable ultra-violet coefficients, but the large amount of labour spent on the analysis before this work was available made it impossible to repeat it. Since the wrong ozone amounts have been used consistently we do not expect that the results will be greatly altered except that all ozone amounts should perhaps be increased by the factor 1·3 proposed by Walshaw (1957).

The calculation of a large number of band areas from Walshaw's empirical function can be exceedingly tedious, and band areas from the ground to the top of each layer were therefore tabulated in terms of convenient variables. For layers 3 and 4 the total ozone amount and the assumed mean pressure are convenient variables. For layer 1 the band area is a function only of the ozone amount in layer 1, which is the quantity whose value is sought. For layer 2 there are several choices, and we selected the ozone amounts in layers 1 and 2 as the most convenient variables, the latter being simply related to the ozone amount, the mean pressure and the amount in layer 1.

The observations were made at air masses 1, 2, 3, 4, 5 and 6, with some repeats at opposite azimuths where possible, but the tables were only prepared for air masses 2 and 5 and the values for these two air masses were taken from a smooth curve constructed from the whole set of observations. The reason for this was that in the course of trial computations we found that within the limits of observational accuracy there was no information to be obtained from the shape of the curve of growth but only from its general level. There was therefore nothing to be gained by trebling the work by analysing each observation. This may not be true in a good climate where much more accurate observations are possible.

In preparing these tables a first-order correction was applied for the curvature of the atmosphere. The correction depends on the height of the layer under consideration, which introduces a new variable, so to avoid this, height was identified with a pressure in the I.C.A.O. standard atmosphere and it was assumed that this pressure was the same as the mean pressure used when evaluating band areas using the Curtis approximation (see GR for details). This procedure is not precise, but it is better than neglecting the curvature of the earth, and it can be incorporated without trouble in the tables described above.

3. Results

Between February 1956 and March 1957 a total of 127 sets of observations were obtained, and these were analysed with the help of total ozone amounts supplied by Sir Charles Normand at Oxford. From July 1956 to August 1957 ground-level ozone was measured using the chemical equipment designed by Dr. Ehmert. The chemical measurements were made soon after mid-day at the time of maximum convective activity when they could be expected to be most representative of tropospheric conditions.

Fig. 1 shows the results of the observations. The ozone amounts from 1,000 to 300 mb are given in units of cm STP $\times 10^{-3}$. Points show the individual observations and the full and dashed lines are running means over 10 consecutive days of observation for 1956 and 1957 respectively. Total ozone amounts are shown in the same way but only for days on which emission spectra were also recorded.

The other two curves in Fig. 1 show running means only, since the individual points add little to the argument. The average tropospheric southward wind velocity is evaluated for days of observation only, so that the curve is strictly comparable to the previous two curves. The ground-level ozone amounts are however not comparable since they only partly overlap the spectrographic measurements. So for these a different procedure was adopted and averages were taken over three weeks using observations made every day.

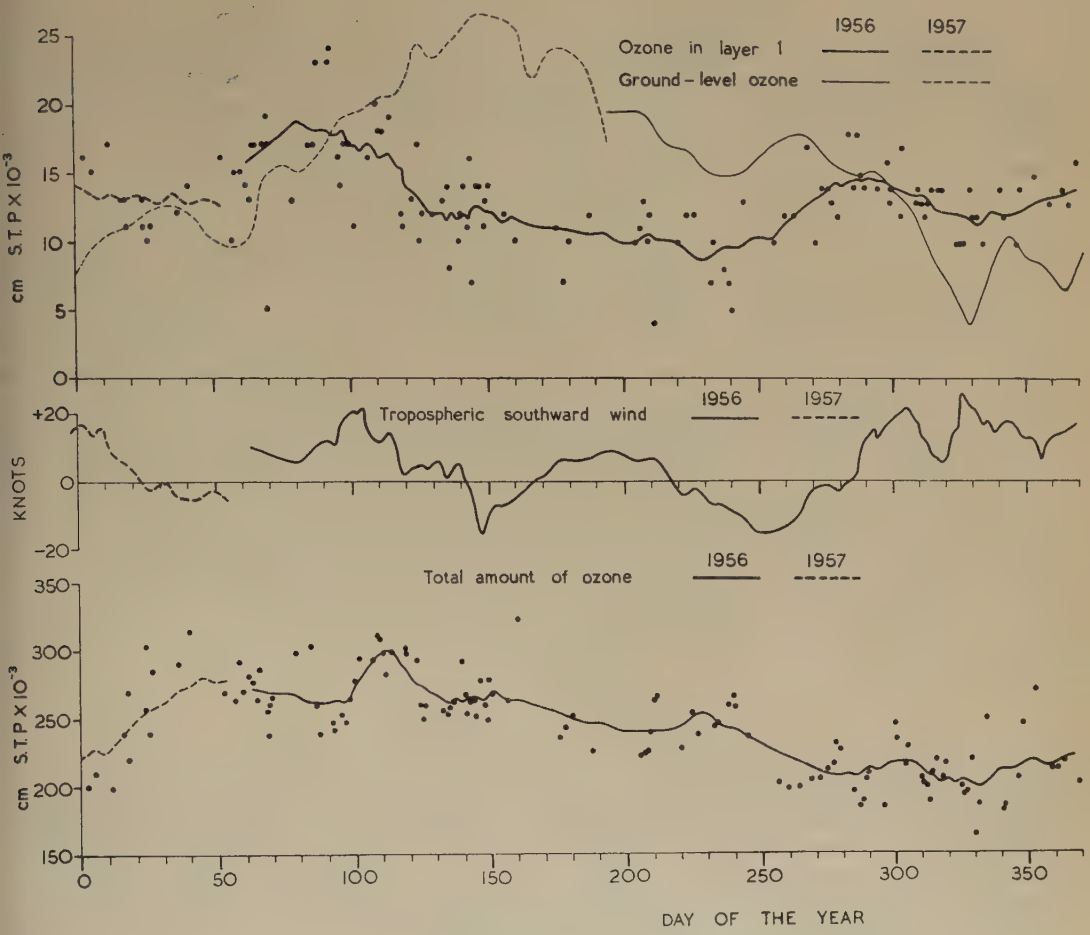

Figure 1. Observations for 1956 and 1957.

The average amount of ozone from 1,000 to 300 mb is $13{\cdot}2 \times 10^{-3}$ cm STP. This agrees reasonably well with the average amount between 0 and 10 km (layer 1 is about 9 km high) given by Paetzold (1955, Fig. 6) from ultra-violet measurements in southern Germany. The two methods are so different that this agreement is very satisfactory.

The amount of ozone in layer 1 is variable, the maximum measured being 24×10^{-3} cm STP and the minimum 4×10^{-3} cm STP. Much of this variation is of rather short period, for the running mean curve shows a change of only two to one, with a maximum in spring and a minimum in late summer. This division into variations of the running mean curve and variations with respect to it is probably significant from a meteorological point of view. The period of averaging is close to a month, which is long compared to the life of a weather system over one particular place, and short compared to a season. The running mean curve should therefore be representative of repeatable seasonal changes, and the short-period fluctuations should be attributable in the main to the passage of weather systems.

In view of the well-known relations between total ozone and weather (Ramanathan 1954, Normand 1953) and since these may be partly due to the downward transfer of ozone from levels where it is rapidly created to levels where it can be stored, it was expected that fluctuations in tropospheric ozone might be closely related to weather changes. An examination of weather charts failed to reveal any such connexion. This does not mean that none exists, but merely that it fails to catch the eye, and it was therefore decided to try a statistical approach.

Correlations have been taken between various elements and between the fluctuations of elements with respect to the running mean curve. The difference between these two is attributable to correlations between running mean curves, which are of course to be seen directly from Fig. 1, so that it is possible to decide whether significant relations have their bases in seasonal changes or weather fluctuations.

TABLE 5. CORRELATIONS BETWEEN VARIOUS ELEMENTS.

	Elements		Correlation coefficient	Degrees of freedom	Comments
1.	δm_1	δm	− 0·17	117	Whole year : hardly significant
2.	δm_1	δm	− 0·43	31	Spring only : highly significant
3.	m_1	$\bar{B}_1$	small	126	Not significant
4.	m_1	$\bar{B}_2$	negative	126	Just significant
5.	m_1	$\bar{B}_3$	− 0·37	126	Significant
6.	δm_1	$\delta \bar{B}_3$	− 0·36	117	Significant
7.	m_1	u	+ 0·13	126	Hardly significant
8.	m_1	m_0	+ 0·10	72	Hardly significant

4. DISCUSSION

(a) *Tropospheric and total ozone*

Discussion of these results* must be made against the background of accepted theories of ozone variations (e.g., Ramanathan 1954 and Paetzold 1955). The picture is inconclusive and a little confused, largely because the only frequently-made observation is the total ozone amount. There is, however, fairly general agreement that photochemical processes control only the supply of ozone to levels below 30 km and that the total amount accumulated and its distribution in space are controlled by the transfer processes and the rate of destruction due to chemical rather than photochemical reactions. These transfer processes may be taking place in the air mass under consideration, or they may have taken place elsewhere (e.g., in the polar regions) with subsequent advection to the latitude of observation. There is good circumstantial evidence in favour of these changes occurring mainly in the region of 10 to 20 km, and that they are in the sense that increases of ozone are caused by downward motions or southward advection and vice versa.

In the light of these ideas, correlations 1 and 2 are unexpected since they indicate that short-period increases in tropospheric ozone associate with short-period decreases in total ozone. Correlation 1, for the whole year, is actually hardly significant, but much of the variance is attributable to the spring period, leading to the significant correlation 2. It is not surprising to find the spring season differing from other seasons. Ramanathan (1954) states that ozone variations associated with weather systems are most marked in the period October to June, and Paetzold (1955) supposes that spring fluctuations are dominated by advection while at other seasons they are dominated by vertical motions and mixing. Our result confirms that fluctuations are probably not due to the same causes at all seasons, and it tends to substantiate Paetzold's supposition, for differences in tropospheric and stratospheric behaviour are more plausibly attributed to advection than to vertical mixing, which would be expected to give rise to similar behaviour at all levels.

As regards the seasonal trends of total and tropospheric ozone, Fig. 1 shows that there is little correlation between them. Both have maxima in the spring but the minima are not closely related.

* It is perhaps hardly necessary to point out that we are making generalizations from 127 days which are a biased selection, for the requirement of a clear sky gives a bias towards anticyclonic conditions. Since we can suggest no reasonable method of allowing for this the discussion proceeds on the assumption that our results are representative.

(b) *Vertical mixing of ozone*

The correlations 3, 4 and 5 show how the tropospheric ozone is related to Planck functions (effectively temperatures) in the layers 1, 2 and 3. These were first investigated graphically, which accounts for the lack of numbers against 3 and 4, but the graphical method showed clearly that there was no correlation with $\bar{B}_1$, significant correlation with $\bar{B}_2$ and a stronger correlation with $\bar{B}_3$. For layer 3 the correlation is just the same for short period and seasonal trends, as shown by the correlation 6, so we may reasonably infer that the same mechanism is responsible for both.

An interesting feature of these correlations is that they seem to establish the correctness of the analytical procedure, for in the analysis m_1 is most associated with $\bar{B}_1$ and least with $\bar{B}_3$ so that if there were errors in the procedure we may expect the strongest correlation with $\bar{B}_1$ rather than $\bar{B}_3$.

Since this is therefore a significant effect, a meteorological cause should exist, and this probably lies in the fact, recorded earlier, that temperatures in layer 3 correlate with temperature gradients in the sense that high temperatures imply great stability. This correlation is an extremely good one and was the basis of the method of evaluating $\bar{B}_3$. Stable gradients mean less mixing, therefore the observed correlation can be interpreted as meaning that strong mixing in layer 3 gives large tropospheric ozone amounts. Since layer 3 is the junction between the region of photochemical creation and the storage region below, this is a reasonable result, and it indicates that a substantial fraction of the variation of tropospheric ozone is due to downward turbulent transfer.

(c) *Meridional transfer*

Many features of the ozone problem have been attributed to meridional transfer, but figures are almost entirely missing on this important topic. It has not been suggested that finite fluxes exist in the troposphere, but it is nevertheless worth while to investigate the tropospheric problem.

Since in the analysis it is assumed that there is a constant density of ozone at all levels in layer 1 it is correct to use the unweighted average of the southward component of the wind through the whole layer. The running mean of this quantity is shown in Fig. 1 ; the average for days of observation is $+ 5{\cdot}7$ kt. This gives rise to a mean southward flux of $1{\cdot}2 \times 10^{14}$ ozone molecules cm^{-2} sec^{-1}. The total flux of ozone due to motions of all kinds, steady and unsteady, can be found by forming the mean value of the product of the ozone density and the wind velocity and it is $1{\cdot}3 \times 10^{14}$ molecules cm^{-2} sec^{-1}. The balance which can be attributed to unsteady motions is obviously small and correlation 7 shows that it is probably not significant.

It is now of interest to attempt a comparison between the rate of supply of ozone to the troposphere by vertical mixing and the rate of supply by advection. Regener (1957) estimates the average vertical flux of ozone near to the ground to be $1{\cdot}2 \times 10^{11}$ molecules cm^{-2} sec^{-1} and Paetzold (1955) estimates the vertical flux in the lower stratosphere to be 5×10^{10}. Considering the uncertainties involved these two figures are remarkably close and suggest a normal vertical ozone flux through the troposphere of 10^{11} molecules cm^{-2} sec^{-1}, and therefore an average rate of supply in the troposphere of 10^5 molecules cm^{-3} sec^{-1}.

To estimate the rate of supply caused by advection we must know the approximate magnitude of the characteristic length which relates the flux to its divergence. This we can only guess. It cannot be much more than 10^3 km, suggesting a rate of supply of the order of 10^6 molecules cm^{-3} sec^{-1}.

This is not a precise argument, but the figures suggest that some of the ozone in the troposphere may owe its existence there to advection. The results of the previous section show that some tropospheric ozone has probably been mixed down from higher levels, so it seems that both factors have to be considered in these latitudes.

(d) *Ground-level chemical measurements*

The relationship between ground-level concentration of ozone and the total amount in the troposphere leads to one of the most puzzling features of this investigation. The chemical equipment was not available when the infra-red series started so that the two sets of measurement do not completely overlap. In Fig. 1 the ground-level measurements are given slightly differently from the others. Results for every day of the year are used and the curve shown is the running mean over three successive weeks. Results for a complete year are shown but they start and stop at day 195, and not near day 60 as for the other observations.

The scale of the ordinate is cm STP $\times 10^{-3}$, instead of the more usual $\gamma\ m^{-3}$. This is to make the spectrographic and chemical results more easily comparable, and the factor $1\ \gamma\ m^{-3} = 0{\cdot}42$ cm STP is based on the assumption that the ground-level concentration remains constant through a layer 9 km deep.

Ground-level chemical measurements are probably strongly affected by local circumstances. The curve shown in Fig. 1 agrees with observers in other locations only in that it shows a maximum in spring or early summer. Of published results, ours most closely resemble those of Götz and Volz (1951) at Arosa, whose maximum is however somewhat earlier and less intense. Our concentrations are smaller through most of the year than those of Wilson (1952) in Alaska, and they are greater than the long Paris series quoted by Fabry (1950). They are difficult to compare with Rasool (1956) for his results show large variations from month to month, attributed to the proximity of Paris (Rasool was placed rather similarly with respect to Paris as Ascot is with respect to London).

The position is complex enough and it is not helped by the comparison with our spectrographic results. There is a tremendous discrepancy between running means about day 150, although it must be pointed out that at this time the two different measurements were made in two different years. The spectrographic measurements can hardly be this much in error, and it is difficult to see why destruction at the ground should cause the ground layers to have an excess of ozone compared with the rest of the troposphere.

Even if we overlook this disagreement on the grounds that the measurements are not comparable, we still have 73 days of simultaneous measurement between day 188 (1956) and day 69 (1957). Over this period the average ozone amounts by the two methods are almost exactly the same, but this must be coincidence for the correlation coefficient between the two, as shown in Table 5, is only significant at the 25 per cent level; however, at least it is positive which indicates the possibility of a sensible relation. Apart from this lack of correlation the running means of the two sets of observations in the region of day 200 show discrepancies which are too large for observational error and of the wrong sign for any catalytic destruction process.

We hesitate to adopt the defeatist course of appealing to a secondary ozone source operative near ground level and being particularly strong near mid-day, but it would certainly resolve our difficulties and a case has been made out by Frenkiel (1955) which seems particularly applicable in the polluted atmosphere of southern England. Our results do not entitle us to draw any positive conclusions on this point, but it must be given further consideration.

5. Conclusions

The general results of attempting these measurements in the quite unsuitable climate of the British Isles has been to restrict our useful results to the total amount of ozone in the layer from 1,000 to 300 mb. Nevertheless, the availability of 127 results over a period of a year has made possible a quantitative discussion of the relation of tropospheric and stratospheric conditions, ozone fluxes and the vertical mixing of ozone. If similar results had been available for the two upper layers, as should be possible at a station with a good climate, most of the existing speculations about the ozone problems could be confirmed or denied, leading to a really firm knowledge of stratospheric circulations.

This suggestion is urged because the method, although crude, is extremely simple to operate given the necessary apparatus, and it provides a complete alternative to chemical methods which are the other major possibility for tropospheric measurements.

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Vanishing potential temperature gradients in strong convection

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Summary

Temperature profiles recorded up to 30 m show an unexpected feature in conditions of free convection with light wind. Above the strong lapse near the ground, there is a transition to zero potential temperature gradient. The height of the transition level falls as the wind strength decreases, becoming less than 8 m when the negative Richardson number at 1·5 m is greater than 0·4. From reported observations over the sea it seems that the feature is present there also.

1. Introduction

The broad characteristics of the temperature profile in the lowest layers of the atmosphere are well known. Under steady synoptic conditions in clear weather, the marked diurnal temperature cycle near the ground diminishes in amplitude (and lags in phase) at increasing heights until it has virtually disappeared at about 500 m (Sutton 1953).

Temperature profiles up to 30 m height have been recorded over flat, open grassland at Edithvale (about 20 mi S.S.E. from Melbourne) over the past few years, as part of the programme of micrometeorological research described by Swinbank (1955). The purpose of the present paper is to describe an unexpected feature that has been found in these profiles in conditions of strong free convection with light wind (large negative Richardson number). Above the strong lapse near the ground, there is a transition to zero potential temperature gradient. The transition appears to be fairly sharp. Its height decreases with increasing negative Richardson number, falling to below 8 m in the lightest winds.

2. Observations

Temperatures are recorded with platinum resistance elements mounted at heights of $\frac{1}{2}$, 1 and 2 m on a portable mast and at 2, 8 and 30 m on a tower built as an open framework of steel pipes. Each element is in an aspirated radiation shield of two chromium-plated concentric cylinders. The elements are connected in pairs in Wheatstone bridge circuits to record temperature differences, in each case from a reference height of 2 m. In the earlier stages (1951 and 1952) the recording equipment was not in operation for a number of the runs, and Assman psychrometers were then used instead.

The period covered is November 1951 to April 1957. There are 111 runs included, mostly of 5-min duration with a few of 10 min, commencing at times ranging from 0927 to 1652 Aust. EST (local mean noon is at 1220 AEST).

It is necessary to avoid the unsteady conditions in the early morning or late afternoon, or due to broken cloud causing temperature inhomogeneities over the ground surface; so all runs at times before 0900 and after 1700, and all runs with low or medium cloud of 4/8 or greater cover, have been excluded from the analysis.

The potential temperature difference, averaged over the duration of each run, will be denoted by $\theta_2 - \theta_1$, for example; the subscript indicating the height z in metres. The wind and potential temperature gradients at 1·5 m, $\left(\frac{dU}{dz}\right)_{1\cdot5}$ and $\left(\frac{d\theta}{dz}\right)_{1\cdot5}$, have been obtained from the readings at $\frac{1}{2}$, 1, 2 and 8 m (or at however many of these heights were

available) by E. L. Deacon's (1953) method of objective interpolation. Thence the Richardson number $Ri = \frac{g}{\theta}\frac{d\theta}{dz}\Big/\left(\frac{dU}{dz}\right)^2$ at 1·5 m has been calculated for each run. In cases with $-Ri_{1\cdot5}$ greater than 0·2, the potential temperature gradients have been recalculated using readings at $\frac{1}{2}$, 1 and 2 m only, and the correspondingly revised values of $Ri_{1\cdot5}$ adopted. This procedure is desirable in view of the results to be described; generally the revised values of $Ri_{1\cdot5}$ do not differ greatly from the originals.

Only unstable stratification at 1·5 m, that is $Ri_{1\cdot5}$ negative, is considered. For convenience, the magnitude $-Ri_{1\cdot5}$ is denoted by $\mathscr{R}$.

3. Results

The results are displayed in Fig. 1. The ordinate is $\theta_{30} - \theta_8$, averaged after grouping the data in 14 equal ranges of the abscissa, which is $\mathscr{R}$ on a logarithmic scale since this gives a fairly uniform distribution of the data.

The unexpected feature of Fig. 1 is that as $\mathscr{R}$ increases beyond about 0·05, $|\theta_{30} - \theta_8|$ decreases, reaching zero at about $\mathscr{R} = 0{\cdot}4$ and (within the statistical scatter) remaining there. It is found that this decreasing trend is not accompanied by any general decrease in $\left|\left(\frac{d\theta}{dz}\right)_{1\cdot5}\right|$ values; so the apparently diminishing gradient does not extend to the whole temperature profile, but is confined to the upper levels. This is demonstrated in Fig. 2, where an indicator of profile shape, the ratio $(\theta_{30} - \theta_8)\Big/\left(\frac{d\theta}{dz}\right)_{1\cdot5}$, is plotted against the $\mathscr{R}$ grouping (the ordinate has been divided by 1·5 ln (30/8) to make it non-dimensional and equal to unity for the logarithmic profile). As $\mathscr{R}$ increases above 0·08, the ratio decreases from the value representing Priestley's (1954a) free convection profile $\frac{d\theta}{dz} \propto z^{-4/3}$ until it becomes zero for $\mathscr{R}$ greater than 0·4. The behaviour at levels below 8 m is shown in Fig. 3; it is apparent that $(\theta_8 - \theta_2)\Big/\left(\frac{d\theta}{dz}\right)_{1\cdot5}$ decreases from the 4/3 profile value beyond about the same $\mathscr{R} = 0{\cdot}4$ as where $(\theta_{30} - \theta_8)\Big/\left(\frac{d\theta}{dz}\right)_{1\cdot5}$ reaches zero in Fig. 2.

In view of the observational scatter, no great weight can be attached to the single point at $\mathscr{R} = 3{\cdot}0$. However, in Fig. 3 this point lying just below the zero axis does lend support to the downward trend towards this axis – all the other individual $(\theta_8 - \theta_2)\Big/\left(\frac{d\theta}{dz}\right)_{1\cdot5}$ values contributing to the averages plotted in Fig. 3 are well above zero.

The interpretation of these results is that at a certain height there is a transition from the strong potential temperature lapse to virtually zero gradient in the region above. The transition height falls with increasing $\mathscr{R}$, reaching about 30 m when $\mathscr{R} = 0{\cdot}08$ (it is, of course, appropriate to judge this from Fig. 2 rather than from Fig. 1), and reaching about 8 m when $\mathscr{R} = 0{\cdot}4$.

Although our main concern is with large $\mathscr{R}$, it is interesting to consider the behaviour at small and medium $\mathscr{R}$ exhibited in Fig. 2 and 3. The broken curves have been plotted in the following way. Priestley (1955, 1956) has found, from evidence involving measured temperature gradient magnitudes but not profile shapes, that the regime governing the the heat-flux changes fairly sharply from forced convection below to free convection above a certain level. This is at the height where Ri reaches a critical value (found by him to be about $-0{\cdot}03$), so the height increases as $\mathscr{R}$ decreases. Assuming that $\frac{d\theta}{dz}$ conforms to the forced and free profiles z^{-1} and $z^{-4/3}$ in the respective regions, the whole $\frac{d\theta}{dz}$ profile shape becomes theoretically determinate for given $\mathscr{R}$. In this way the theoretical

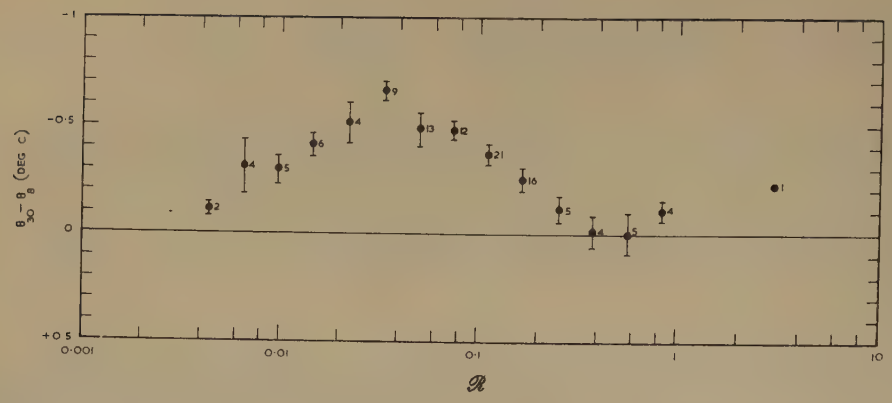

Figure 1. $\theta_{30} - \theta_8$ averaged in ranges of $\mathscr{R}$, which denotes $|Ri_{1\cdot5}|$. Number of values used and standard error of each average are indicated.

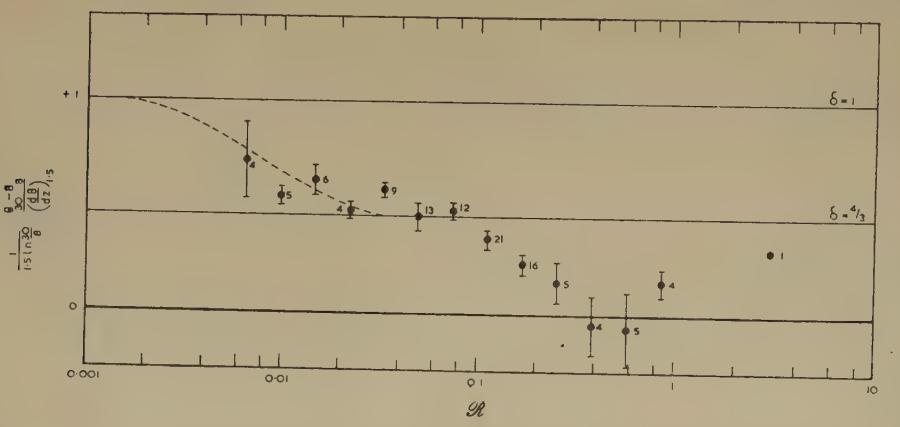

Figure 2. const $\times (\theta_{30} - \theta_8) \Big/ \left(\frac{d\theta}{dz}\right)^{1\cdot5}_{1\cdot5}$ averaged in ranges of $\mathscr{R}$. Number of values used and standard error of each average are indicated. The two straight lines represent $\frac{d\theta}{dz} \propto z^{-\delta}$ for $\delta = 1$ and $\delta = 4/3$.
Broken curve : see text.

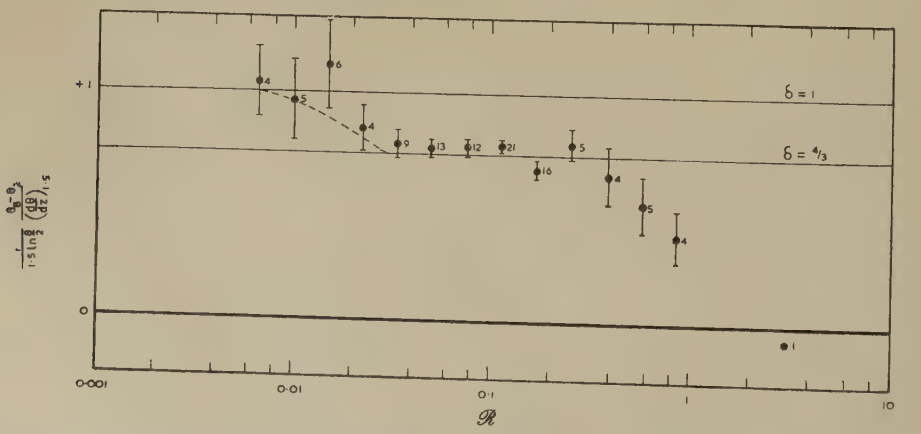

Figure 3. const $\times (\theta_8 - \theta_2) \Big/ \left(\frac{d\theta}{dz}\right)^{1\cdot5}_{1\cdot5}$ averaged in ranges of $\mathscr{R}$. Number of values used and standard error of each average are indicated. The two straight lines represent $\frac{d\theta}{dz} \propto z^{-\delta}$ for $\delta = 1$ and $\delta = 4/3$.
Broken curve : see text.

broken curves have been plotted. It will be seen that (to within the scatter) the experimental points conform with these curves and thus give independent evidence for the correctness of the profile predicted by Priestley's identification of the forced and free convection regions. At low $\mathscr{R}$, some of the points averaged in the ordinates of Figs. 2 and 3 represent the ratio of two small values, so the experimental error is magnified and the scatter is much larger than in Fig. 1. This is particularly so in the lowest $\mathscr{R}$ range, which has therefore been omitted from Figs. 2 and 3.

The medium $\mathscr{R}$ range between the right-hand end of the broken curve and the commencement of falling ordinate values (i.e., between 0·03 and 0·08 in Fig. 2, between 0·03 and 0·4 in Fig. 3) represents purely the free convection regime below the transition to neutral stratification. In this range the plotted points strongly indicate Priestley's $\frac{\partial\theta}{\partial z} \propto z^{-4/3}$ profile.

Two facts indicate that at the transition between the superadiabatic region and the overlying neutral region, the boundary is rather sharp. Firstly, the points in Fig. 3 start to fall below the $\delta = 4/3$ line at almost the same $\mathscr{R}$ as where the points in Fig. 2 reach zero. Secondly, the ends of the range of downward trend in Figs. 2 and 3 are rather sharply defined.

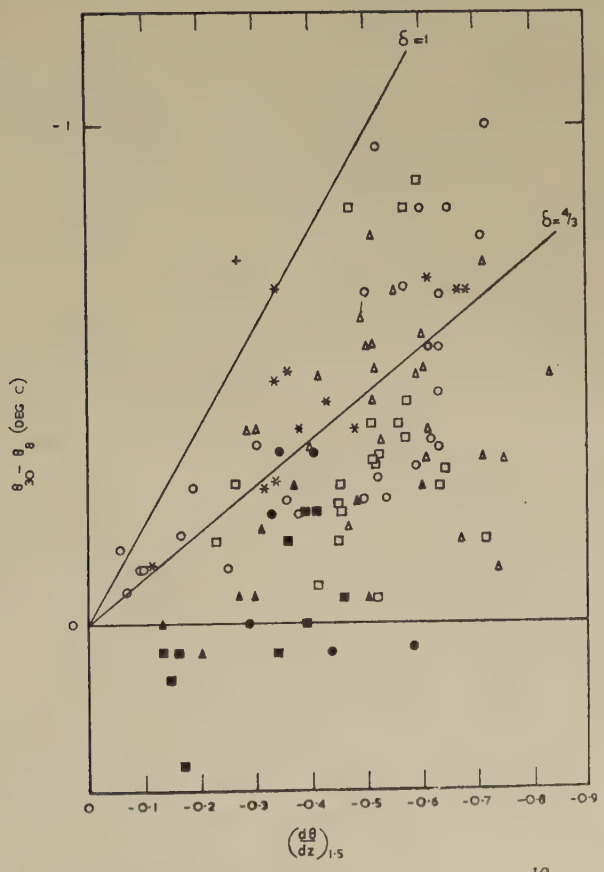

Figure 4. $\theta_{30} - \theta_8$ plotted against $\left(\frac{d\theta}{dz}\right)_{1\cdot5}$. Straight lines represent $\frac{d\theta}{dz} \propto z^{-\delta}$ for $\delta = 1$ and $\delta = 4/3$.

Heavier symbols represent weaker wind shear, ranges of $\left(\frac{dU}{dz}\right)^2_{1\cdot5}$ being as follows ($\sec^{-2}$):

■ ⩽0·02	△ 0·16 to 0·32
▲ 0·02 to 0·04	○ 0·32 to 0·64
● 0·04 to 0·08	✱ 0·64 to 1·28
□ 0·08 to 0·16	+ > 1·28

For $\mathscr{R} > 0{\cdot}05$, detailed inspection shows that increasing $\mathscr{R}$ is strongly associated with decreasing $\left(\frac{dU}{dz}\right)^2_{1{\cdot}5}$ while $\left(\frac{d\theta}{dz}\right)_{1{\cdot}5}$ remains fairly constant (for $\mathscr{R} > 0{\cdot}2$, the wind was light, between 1 and 2 m/sec at 2 m height in nearly every case). Hence the regular downward trend of Fig. 1 is not to be expected if $\left|\left(\frac{d\theta}{dz}\right)_{1{\cdot}5}\right|$ is taken instead of $\mathscr{R}$ as abscissa. In Fig. 4, $\theta_{30} - \theta_8$ is plotted against $\left(\frac{d\theta}{dz}\right)_{1{\cdot}5}$ with a point for each run. The points cluster very broadly about the line $\delta = 4/3$. However, there is a marked tendency for the points with lightest wind shear (heaviest symbols) to lie below this line, near or even below the $\theta_{30} - \theta_8 = 0$ axis. These represent the runs with the largest $\mathscr{R}$ values in Fig. 1.

When points for the 111 individual runs are plotted on the same scales as in Figs. 1, 2 or 3, there does not appear to be any systematic relationship between the scatter of ordinate values and either the distinction between morning and afternoon runs or the wind direction. Thus the relationship found is independent of whether the ground temperature is increasing (before noon) or almost stationary or decreasing (after noon); and is not affected by the surface inhomogeneities and obstacles which exist in various directions beyond about a mile from the observing site.

A few measurements at heights from 30 to 100 m (temperature difference from 2 m) have been recorded at Edithvale, using the kite-balloon equipment of Deacon, Sheppard and Webb (1956). For cases when the temperature transition would be expected, from the trend of Fig. 2, to be at about 30 m or somewhat higher, the kite-balloon results confirm the disappearance of the lapse above. For lower transition levels, that is weaker winds, this equipment is unfortunately not suitable, since its aspiration is only that provided by the wind.

4. Comparison with published results

The vanishing of potential temperature gradient only a few metres above the ground in convection with light winds does not appear to have been reported previously. However, some published results indicate what might be the same feature at heights between 30 and 100 m, and reported temperature profiles over the sea suggest that the feature is present there also. These are discussed below.

Mal, Desai and Sircar (1942) quote average temperature lapse in the two layers between heights 4, 56 and 156 ft at Karachi aerodrome. For clear days in the hot season (April) it will be seen that in the lower layer there is maximum strong lapse between noon and mid-afternoon; but in the upper layer the lapse has a broad trough at about noon, between peaks at 10 hr and 16 hr. A similar feature is apparent in other months also. The same kind of behaviour is evident to a minor extent in the data for Ismailia reported by Flower (1937). Again, in the July observations of Best, Knighting, Pedlow and Stormonth (1952) in south-east England, the lapse in the layer 47·2 to 106·7 m has a trough at 13 hr while the lapse at lower levels reaches a maximum at this time. These last authors point out that the effect might be due to a sea breeze and calls for further investigation.

Frankenberger's (1953) profiles of potential or equivalent potential temperature averaged for clear summer days at a site in northern Germany show zero or even reversed $\frac{d\theta}{dz}$ above 30 to 70 m when the wind is light. When $\frac{d\theta}{dz} \propto z^{-\delta}$ is fitted (Frankenberger 1955), the values of δ exhibit the corresponding behaviour, viz., they become large at the greater heights in light winds. Similarly, in an earlier analysis of Leafield data, Deacon (1948) estimated δ from the mean observed ratio $(\theta_{30{\cdot}5} - \theta_{12{\cdot}4})/(\theta_{12{\cdot}4} - \theta_{1{\cdot}2})$ in summer strong lapse conditions, and found δ larger in light winds. His results are reproduced in Table 1 together with rough estimates of $\mathscr{R}$ supplied by Deacon (private communication); it is apparent that they agree closely with the present results in Fig. 2.

TABLE 1. DEACON'S (1948) ESTIMATES OF δ

13 m wind (m/sec)	1·5-3	3-4	4-6	6-8	8 +
Approximate $\mathscr{R}$	0·12	0·08	0·04	0·02	0·01
Index δ	1·53	1·46	1·33	1·30	1·23

The conditions of large $\mathscr{R}$ approach those favourable for the formation of dust devils. In a paper on the latter subject, Ives (1947) quotes temperatures at several heights on an occasion of light wind; the corresponding potential temperature gradient will be found to vanish above about 30 m.

A relationship similar to that in Fig. 4 has been observed over the sea by Johnson and Meredith in 1927 and by Deacon, Sheppard and Webb (1956) – for details see the latter paper. In both these cases, the abscissa is air-sea temperature difference rather than $\frac{d\theta}{dz}$ in the air. The appearance of their results is very similar to that in Fig. 4, with the occasions of light wind tending to have zero or positive $\theta_{\text{upper}} - \theta_{\text{lower}}$. Thus a relationship similar to that in Figs. 1, 2 and 3 presumably applies over sea as well as over land.

An apparent transition in the profiles plotted by Brocks (1948) is due only to the method of plotting. Brocks plots temperature gradient against height on log-log scales, using averaged data from several sites. Large $\mathscr{R}$ and small transition heights would not be expected in his averaged data, so it might seem surprising that his plots are characterized by an apparent sharp transition to near-adiabatic values above a height between 10 and 100 m. However, there is no actual transition in the atmosphere at this height; the effect is introduced by plotting the gradient of temperature rather than of potential temperature on a logarithmic scale, when actually the potential temperature gradient decreases steadily with increasing height. When Brocks's points are re-plotted as potential temperature gradients, their trend is smooth (except that the experimental scatter becomes magnified for small values plotted on a logarithmic scale) up to the greatest heights quoted (200 m or so) with no suggestion of a discontinuity.

5. Discussion

It is believed that the decrease of transition height with increasing $\mathscr{R}$ (diminishing 1·5 m wind and wind shear) leads, as outlined in the next paragraph, to a rather fundamental conclusion concerning the mechanism responsible for the mixing of heat between buoyant element and environment. It is that this mixing is due mainly to turbulence induced by the wind shear (this is the regime envisaged by Priestley (1953)), and not appreciably to turbulence induced by the buoyant motion itself.

For given turbulent diffusivity, larger elements have smaller rates of mixing. At increasing heights, one expects to find larger elements starting, until a particular height is reached where the elements are large enough to mix negligibly with the environment (cf., Priestley's (1953) ' absolutely buoyant ' elements). The smaller the wind shear and consequent diffusivity, the lower will be that particular height. Beyond that height the elements rise with almost constant potential temperature. Therefore, since the *quantity* of heat carried up by an individual element depends only on its temperature excess over the environment, for the heat flux to remain constant with height (steady conditions) the environment must have potential temperature constant with height. The average difference of potential temperature between vertically-spaced instruments will then be almost zero. It is intended to publish elsewhere a quantitative treatment developed along these lines.

If this formulation is valid, then the magnitude of the temperature fluctuations should be almost independent of height above the transition. In the present data, temperature fluctuations were recorded at 16 m and 30 m during three runs having the transition

near 8 m; the ratio of the root-mean-square σ_T at 16 m to σ_T at 30 m was 1·10, 1·04, 1·04 respectively. Thus, so far as this meagre evidence can show, the dependence of σ_T on height is observed to be weak above the transition; on fitting $\sigma_T \propto z^{-p}$, the corresponding values of p are 0·15, 0·06, 0·06 respectively.

At heights above the transition we have an example of upward heat transfer through a region of neutral stability. Buoyant elements can provide this, as was pointed out by Priestley and Swinbank (1947) and treated theoretically by Priestley (1954b). The same thing can be found over the sea (Deacon, Sheppard and Webb 1956). At greater heights in the troposphere, upward heat transfer by buoyant elements through a neutral or stable region is, of course, of frequent occurrence; it can be particularly marked when an air mass is being modified by a warmer surface beneath, as in the case described by Bunker (1956).

A question of interest is whether the wind profile exhibits any peculiarity near the height of the transition in the temperature profile. From inspection of the wind profiles plotted up to 32 m, the conclusion is clear that there is no such peculiarity; the wind profile above the temperature transition is generally a smooth continuation of that below it. Thus there is certainly not similarity between the wind and temperature profiles.

Obukhov (1946) and Deacon (1955) – see also Monin and Obukhov (1954) – have defined as a stability parameter a length scale

$$L = -\frac{u_*^3}{kgH/c_p\rho T}$$

where $u_* = (\tau/\rho)^{\frac{1}{2}}$ is the ' friction velocity ' representing the shearing stress τ, H is the upward heat flux, T the mean temperature, k von Kármán's constant, and g, c_p and ρ have their usual meanings. L has the advantage over Ri of being essentially independent of the height, but has the practical disadvantage that the measurement of H and τ is not straightforward. In the present data, values of H and τ are available for a number of the runs. It is interesting that the computed L values are approximately equal (they are always within a factor of 2) to the estimated heights of transition to neutral stability; which are of the order of thirty times the heights of Priestley's (1955, 1956) change from forced to free convection.

6. Conclusions

Observed temperature profiles for reasonably steady conditions of free convection (Richardson number at 1·5 m negative, $\mathscr{R}$ denoting its magnitude) lead to the following conclusions.

At the top of the superadiabatic layer there is an apparently fairly sharp transition to neutral stratification which is only a few metres above the ground in light winds. It falls with increasing $\mathscr{R}$, being at about 30 m when $\mathscr{R}$ is 0·08, and at about 8 m when $\mathscr{R}$ is 0·4 (the latter corresponding to winds of 1 or 2 m/sec at 2 m).

It appears that when there is a superadiabatic layer over the sea surface, a similar transition occurs there also in light winds. Thus in light winds over land or sea there is upward transfer of heat through a neutrally stratified region only a few metres above the surface.

The transition is thought to be related to reduced mixing between rising elements and the environment at low wind speeds. It seems that this mixing is predominantly due to the wind shear, not to turbulence induced by the buoyant motion.

The wind profile does not show any irregularity near the height of the transition to neutral temperature stratification.

In the superadiabatic layer below the transition, the data strongly support Priestley's free convection law $\frac{d\theta}{dz} \propto z^{-4/3}$ down to the height where $Ri = -0{\cdot}03$. Below this, in conformity with Priestley's finding that the heat flux is then governed by forced convection, the data support the appropriate profile $\frac{d\theta}{dz} \propto z^{-1}$.

Acknowledgments

The writer is indebted to Mrs. N. F. McNally for assistance in scrutinizing the data for this investigation. Most of the temperature-profile measurements were made by Mr. N. G. Richards.

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William Bulkeley's record of the weather of Anglesey 1734-1743, 1747-1760

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Summary

The study of climatic fluctuations before the nineteenth century is hindered by the paucity and poor distribution of instrumental records, and increasing reliance must be placed upon diaries and other descriptive sources. The diary of William Bulkeley of Anglesey, over the period between 1734 and 1760, has been examined. From the information it contains, resultant wind directions and constancy have been calculated. Frequencies of days with rain, snow, hail, frost and thunder are tabulated. A comparison is made between the general monthly rainfall serial values calculated for England and Wales and data derived from the diary. The record of the aurorae observed is given.

1. Introduction

Instrumental recording in Wales was largely delayed until the nineteenth century and before this period there is a virtual blank in climatic information. Hardly any instrumental records have yet come to light, and no thorough study has so far been made of the descriptive documentary material. The writer is at present making such an investigation. Information from Wales or other parts of the seaboard of the British Isles would provide valuable material for comparison with that available from central and eastern England, upon which much earlier work has been based. More light may thus be thrown upon the contrasting effects on the eastern and western areas of the British Isles of major modifications in wind direction in the past.

Few British stations have a consistent, standardized record stretching back more than 100 years. In earlier centuries the material upon which the reconstruction and analysis of past weather and climate can be made becomes much less accurate. By the first half of the eighteenth century instrumental recording had attained only a limited development, and the information it provided was spasmodic and sparsely distributed over the country, as is revealed, for example, by the limited availability of records which could be used by Manley (1953) in his computation of the monthly temperatures of central England from 1698. The serial monthly rainfall values expressed as percentages of the 1881-1915 mean, which have been computed from 1727 onwards, were based largely on two, and at the most four, records in the first half of the eighteenth century (Glasspoole 1928). The diminishing reliability of the information from any sort of instrumental recording in the eighteenth and seventeenth centuries increases the value of descriptive accounts of the weather such as can be found in manuscript diaries.

It is for this reason that the following detailed examination of the diary of William Bulkeley of Anglesey has been made. This journal which has been the subject of study by social and economic historians (Evans 1936, 1953) has not been closely investigated by climatologists. Bulkeley, who lived from 1691-1760 at Brynddu House (O.S. Nat. Grid Ref. 373,913), Llanfechell, was a gentleman farmer. We are fortunate in having extant two out of three volumes of his diary covering a period from 11 April 1734 to 28 September 1760. The middle volume over the period 20 June 1743 to 11 August 1747 is missing. The diary is in manuscript although limited portions, omitting much of the weather record, have been transcribed (Owen 1931). The total length of the diary is 1,050 pages and all daily entries commence by giving the wind direction followed by details of the weather of the day. Whereas Bulkeley never misses a comment upon the weather, the other detail in his daily journal on farm and personal affairs appears only on some days.

The diary is remarkable for the fact that only two days are omitted over the whole time to which it refers. It is also significant that, apart from two visits to Dublin which he made in the early years, one lasting from 7 April to 21 April 1735 and the other from 23 October 1735 to 9 January 1736, Bulkeley rarely travelled far afield. Most of his short absences from home were to visit relatives and friends in other, nearby parts of Anglesey or to attend the Quarter Sessions at Beaumaris. The continuity of the account is not, therefore, disrupted to any significant degree. Bulkeley's avoidance of exaggeration and his use of reasonably uniform and precise descriptive terms contribute further to the value of the record.

2. Resultant wind directions and constancy

William Bulkeley's house was situated just over two miles from the N and NW coasts of Anglesey on the low coastal plateau nearly 100 ft above sea level. He was well placed to observe wind and weather. No major relief feature shelters the house. The highest ground, some 600 yd to the NE of the house, rises to 150 ft, but otherwise the uniform general relief of the island gives excellent exposure and visibility in all directions. NW Anglesey is particularly open to oceanic influences, but is sheltered by the Snowdon region to the SE. Bulkeley carefully recorded wind directions to a scale of 16 compass directions. How he determined direction so precisely is not clear. He had taken considerable interest in and had contributed towards the cost of setting up a weather vane on Llanfechell church in 1737 but this was not close enough to his house to be clearly visible. Whether or not he had a weather vane on his own house is not clear.

When the wind was blowing steadily he recorded one compass direction for that day, but on occasions when a change in direction occurred, such as the passage of a well-marked front, he gives the new direction. In tabulating the daily wind directions according to 16 compass points, for each month of the record, on those days for which a single direction entry was given the specified direction was given a value of two. On days when more than one direction was mentioned, the morning and evening values were taken and each given unit value. Since Bulkeley was generally precise about the times of wind change, even to hours of the day in many cases, this method was not considered unduly subjective.

From the wind frequencies derived from Bulkeley's diary for 16 points of the compass, the resultant wind direction and wind constancy were determined according to the method given by Brooks and Carruthers (1953). The average wind speed Va, which was used in calculating the wind components, was assumed to be given by the relationship $Va \propto f(f^{\frac{1}{2}})$ where f is the wind frequency for a specified direction. The wind constancy q was provided by the expression $q = V_R/V_S$, where V_R is the module of the vector mean wind and V_S is the scalar mean wind. The annual, winter (December to February) and summer (June to August) values of direction and constancy are in Table 1. For comparison, values for the same periods calculated by Brooks (Brooks and Hunt 1933, Brooks 1934) for Dublin, Richmond (Surrey), Edinburgh, Sheffield and Perth are also given. It may be noted that Brooks employed at that time Lambert's method of determining the vector wind values. Unit velocity was assumed for each individual wind observation and eight compass directions were used. A check was made to investigate whether the values determined from the Bulkeley record using Lambert's method were markedly different from those shown in Table 1. It was found that the resultant direction values differed at the most by only a few degrees, but that the constancy percentages from Lambert's method were lower, sometimes by several per cent, than the values in Table 1. The constancy values for Brynddu generally exceed those for the other places given. This may be explained in part by the more open exposure of Brynddu to all directions except the SE.

The resultant direction and constancy values for the period 1735-1759, omitting the incomplete or missing years 1743-1747, were 212° and 24 per cent. The annual values for Anglesey in Table 1 indicate that a resultant direction about SW or SW by S was most common apart from the exceptional years of 1740 and 1742. From 1748 to 1759 the

TABLE 1. RESULTANT WIND DIRECTION (DEGREES AND CONSTANCY (PER CENT)) FOR ANGLESEY WITH COMPARATIVE VALUES FOR DUBLIN, RICHMOND, EDINBURGH, SHEFFIELD AND PERTH

Year	Winter Dir.	Winter Con.	Summer Dir.	Summer Con.	Year Dir.	Year Con.
			(a) Anglesey			
1734	–	–	244	21	–	–
1735	256	55	243	31	226	34
1736	209	49	168	11	219	24
1737	252	66	296	28	255	26
1738	220	57	234	62	217	44
1739	224	63	262	44	240	31
1740	75	30	237	31	311	7
1741	215	29	249	58	224	28
1742	195	19	278	21	327	11
1743	180	34	–	–	–	–
1748	116	28	211	31	217	20
1749	221	53	217	41	200	31
1750	198	60	200	58	186	33
1751	175	25	190	54	185	29
1752	206	11	211	57	213	42
1753	219	20	202	26	210	29
1754	236	32	231	57	220	34
1755	169	35	211	16	218	27
1756	210	65	212	45	210	29
1757	156	35	100	18	154	19
1758	165	27	199	41	159	28
1759	189	43	209	47	187	31
1760	156	38	173	26	–	–
			(c) Richmond			
1734	250	36	240	18	250	25
1735	224	40	240	45	219	23
1736	185	28	270	3	242	16
1737	257	50	300	29	243	11
1738	214	38	260	49	235	24
1739	237	53	230	41	220	20
1740	80	22	240	33	270	4
1741	165	13	231	34	177	13
1742	180	15	110	16	95	10
1743	189	29	150	19	185	15
			(e) Perth			
1754	–	–	232	34	242	24
1755	280	19	225	33	237	28
1756	233	68	175	27	231	22
1757	246	36	191	48	228	31
1758	256	34	218	26	218	16
1759	234	47	224	62	226	33
1760	252	18	225	26	228	32

Year	Winter Dir.	Winter Con.	Summer Dir.	Summer Con.	Year Dir.	Year Con.
			(b) Dublin			
1734	231	53	228	25	239	31
1735	264	56	260	50	245	27
1736	212	16	189	25	216	19
1737	270	45	309	14	252	13
1738	227	42	257	40	228	24
1739	252	43	255	12	259	16
1740	156	10	233	5	13	13
1741	124	8	273	22	207	2
1742	204	12	157	28	203	13
1743	209	39	249	14	219	26
1748	165	24	157	13	233	20
1749	220	33	263	41	244	32
1750	220	40	246	36	225	17
1751	212	38	198	22	238	25
1752	240	19	253	30	236	18
1753	238	42	281	15	261	26
1754	268	59	275	22	262	21
1755	125	28	275	37	258	24
1756	255	51	214	17	220	16
1757	195	23	236	23	249	26
1758	250	29	273	43	234	13
1759	231	35	256	34	265	22
1760	243	13	286	22	261	31
			(d) Edinburgh			
1734	238	81	290	13	243	39
1735	243	62	259	22	251	29
1736	205	23	–	–	–	–
			(f) Sheffield			
1734	238	51	267	32	247	36
1735	263	55	255	33	261	25
1736	291	9	270	5	271	18
1737	276	63	303	36	279	25
1738	242	35	270	46	252	25
1739	259	54	250	42	263	22
1740	21	18	248	36	309	35
1741	306	26	243	42	277	25
1742	283	38	278	30	300	23
1743	229	30	262	25	267	29

Anglesey values reveal an increased tendency to winds from a more southerly quarter than in the earlier part of the record. 1750 and 1751 show the southerly trend clearly, whilst 1757 and 1758 are noteworthy for SSE resultant winds. Whilst 1740 and 1742 are clearly abnormal years in the other records for Dublin and Richmond, there is only a faint hint of the more southerly trend of 1757 and 1758 in the Perth values. The low constancy percentages of the three most unusual years at Brynddu (1740, 1742 and 1757) are noteworthy.

The winter values (December to February) for Brynddu show that the years with the largest winter differences from the average are usually those with the biggest annual anomalies. In addition 1743, 1748, 1755 and 1760 show significant departures. The cold winter of 1740 is revealed by the NE resultant wind, but 1741, also a cold winter, does not show the same differences from the normal pattern in Anglesey as appear in the Dublin and Richmond records. In the record of temperatures for central England (Manley 1953), 1742, 1748, 1751, 1752, 1755, 1757, 1758 and 1760 have relatively cold winters. Of these only 1742 and 1752 in Anglesey do not show an unusual easterly component in the resultant winds. The tendency to resultant winds from east of south is apparent in 6 years out of the period 1748-1760, although the Dublin record, the only complete one for the same period, reveals this only in the years 1748, 1755 and 1757. The first five years of the Anglesey record contrast as a group with the later years in showing a greater westerly characteristic and a higher constancy figure.

The summer record (June to August) shows a different set of variations. It is not 1740 but 1736 which has the most exceptional resultant direction. 1736, 1757 and 1760 all appear as having relatively warm summers in Manley's record. It is of interest to note that in these three years the resultant direction showed a greater easterly component, but, except for 1757, the dominant direction was southerly. Within the period under consideration these years make up three of the four warmest summers in central England. The one exception, 1759, does not show any easterly component but the southerly trend is fairly strongly marked.

These comparisons with Manley's figures imply a parallelism between the conditions in Anglesey and those of central England. Such an assumption, as already indicated, is not necessarily valid. Much depends on the basic cause of the weather phenomena, and the importance or otherwise of situation and exposure in changed patterns of atmospheric circulation. There would appear to be a much closer parallel between unusual Anglesey resultant wind directions and the thermal régime calculated for central England than there is between these wind directions and the general rainfall values over England and Wales expressed as annual percentage values of the 1881-1915 normal (Glasspoole 1928). In the years in question exceptional annual rainfall occurred in 1736 (wet), 1738 (dry), 1740 (dry), 1741 (dry), 1742 (dry), 1750 (dry) and 1751 (wet). In terms of wind direction there does not appear to be any close correlation between the annual resultant winds for Anglesey and the exceptional rainfall years for the country as a whole. Comparing 1736 and 1738, or 1751 and 1750, each pair being ' wet ' and ' dry,' there is little difference in resultant directions and, in the latter pair, little difference in constancy. The percentage differences (in terms of the 1881-1915 normal) between the wet and the dry years in each of these two pairs of years, according to Glasspoole, were 32 per cent and 41 per cent respectively. 1740-1742 were abnormal years in a number of respects but the characteristics of the Anglesey resultant wind direction would not lead one to expect that 1741 would be the driest of the three years.

3. Days with rain

The daily weather entry of the diary permitted the preparation of a month-by-month total of days on which precipitation fell. The results are set out in Table 2. In all years the dates of the diary have been corrected to the New Style Calendar. It was possible in a large number of entries to determine the actual day in which the fall should be included since Bulkeley often gave the time of the day when rain either began or ended. Where rain, hail or snow fell before 9 a.m. it was included in the rainfall day ending at 9 a.m., but where rain was recorded as falling from before 9 a.m. to some time later in the next rainfall day, that fall was counted as two days with rain, as would be done in the tabulation procedure adopted in the present-day *Monthly Weather Report*. Sometimes rain was recorded ' in the morning ' or ' all the morning ' with no further details. In these circumstances, whilst the fall might well have occurred on parts of two rainfall days, it was

considered too subjective to try to estimate from the text of the diary whether or not such was the case. Consequently such falls were assumed to be after 9 a.m. on the day in question and were counted as one day with rain. One other problem arose from the few entries where it was stated that ' little or no rain fell '. Since even small falls and brief showers were mentioned by Bulkeley, it was considered that such an entry should be interpreted as no rainfall. The totals of ' days with rain ' include all days when precipitation in any form was recorded.

TABLE 2. Weather record for Brynddu

		Days			
Year	Rain	Snow or sleet	Hail	Thunder	Frost
1735	177	4	2	3	15
1736	157	15	7	2	18
1737	147	5	8	3	15
1738	190	8	10	4	13
1739	182	14	16	2	16
1740	145	21	15	6	82
1741	131	7	8	1	43
1742	122	14	10	2	53
1748	175	21	9	4	38
1749	192	21	10	3	17
1750	166	18	18	2	21
1751	164	11	11	3	33
1752	165	14	11	4	22
1753	162	8	14	3	33
1754	168	21	15	2	27
1755	190	20	20	2	37
1756	175	18	14	4	23
1757	149	15	12	2	41
1758	168	11	9	1	24
1759	153	12	5	2	19
Mean	164	14	11	3	29
		Mean frequency at Valley, Anglesey			
1942-1956	189	12	13	5	50

One of the major problems in attempting to obtain the frequency of rainfall from descriptive accounts is the under-recording of falls at night. In this connection Bulkeley seems to have been very observant, frequently giving the hours at which rain occurred during the night, though how he got this information, unless one assumes him to be a poor sleeper and constantly on the watch for rain, is not clear. However, some falls of a minor character at least must have gone unnoticed at night. Because of this and the other problems referred to above the annual totals of days with rain at Brynddu are probably on the low side. This certainly appears to be so when comparing them with the Holyhead totals of rain-days available in the *Monthly Weather Report*. For this reason quantitative comparisons between the figures given in Table 2 and those determined from precise rain-gauge records are of little value. It is possible, however, to get some indication of the relative wetness or dryness of years or months within the period of the diary. By combining the descriptive comments about the nature of the precipitation with the number of days with rain, it is possible to get a reasonable pointer at least to those months which are unusually wet or dry. The monthly rainfall serial values, expressed as percentages of the 1881-1915 normal in *British Rainfall* (Glasspoole and Nicholas 1931), were compared with the appropriate months of the diary record. An exact correspondence would

not be expected. The earlier monthly rainfall serial values were produced with reference to at the most, four, and usually two, records. The records were from English stations and, with the exception of Huxham's Plymouth record, not very representative of a western seaboard climate. Glasspoole emphasizes the increasing unreliability of the earlier serial values in his table.

Some interesting contrasts appeared between the serial values and the Brynddu record. For about 27 per cent of the months it was not possible to come to any firm decision whether the Brynddu conditions were in agreement or not with the serial values. 29 per cent of the months were dry and 26 per cent were wet in both records. The remaining months, 50 in number, were strongly at variance with the serial record. They are given in Table 3. With hardly any exceptions, the months drier at Brynddu are those of spring or summer, whilst the majority of the wetter months are the wet winter months of the western coasts. Space does not permit the quotation of the passages of the diary and of the number of days with rain upon which these conclusions have been based. Necessarily the decisions must be somewhat subjective, and it is for this reason that only the strongly discrepant months have been given.

TABLE 3. Months at Brynddu with rainfalls probably different from the serial values for England and Wales

Year	Wetter than serial values	Drier than serial values
1734	July, Oct.	—
1736	Nov.	—
1738	Jan., Feb., March, July, Oct., Nov., Dec.	—
1739	—	April
1740	—	April
1742	—	April, May, June
1743	Feb., April	—
1748	May, Aug., Sept., Nov.	—
1749	Aug., Nov., Dec.	June
1750	Feb., Aug.	May, June
1751	—	June, July
1752	Sept., Oct., Nov.	—
1753	March	July
1754	Oct., Nov.	—
1755	Dec.	—
1756	March, Oct., Nov.	—
1757	Nov.	May, Sept.
1758	Dec.	July
1759	Jan.	April, Sept.

4. The weather record

The information in Table 2 represents a digest of detailed monthly tables which space does not allow to be given in full. For comparison, the mean values of the 15-yr period of observations in the *Monthly Weather Report* for Valley, Anglesey, are given. The abnormal years 1740 to 1742 are revealed by the high totals of frosts. In the cold January and February of 1740, 47 days had frosts but owing to the dryness of this period of east winds very snowy weather was not experienced. In the March of both 1741 and 1742 the weather was very dry and relatively frosty with winds from easterly or northerly quarters. The other years with the higher totals of frost days are generally in agreement with the years with the lower winter temperatures in the central England record. The only months outside 1740-1742 which reach double figures in frost frequency are the Januarys of 1755, 1757, 1758, 1760 and the Februarys of 1748, 1751 and 1755. The five years

1735-1739 in many respects show evidence of milder conditions than were experienced for most of the other years of the Brynddu record. Table 4 indicates well the vagaries of the date of the first and last frost and the length of the frost-free period in an individual year. There is little or no correlation between the years which were exceptionally mild or cold and the data shown in Table 4.

The diary includes many interesting general comments upon the weather of particular seasons or years but space does not permit the inclusion of this descriptive material.

TABLE 4. Frost record at Brynddu

Year	Last frost in spring	First frost in autumn	Frost-free period (days)
1734	14 May	19 November	188
1735	1 April	8 November	220
1736	13 May	13 November	183
1737	28 May	14 November	169
1738	20 March	4 October	197
1739	5 May	17 October	164
1740	11 June	5 October	115
1741	13 April	13 December	243
1742	27 July	31 October	95
1743	28 May	—	–
1747	—	6 December	–
1748	25 May	20 October	147
1749	10 June	30 September	111
1750	8 June	2 November	146
1751	2 July	11 October	100
1752	14 April	19 November	187
1753	9 May	25 August	107
1754	1 May	20 November	202
1755	2 June	14 October	133
1756	24 March	7 September	166
1757	6 June	13 August	67
1758	2 May	13 October	163
1759	21 May	16 November	178
1760	21 May	—	–

5. Aurorae

In Table 5 are set out the dates and number of occasions on which the aurora was described in the diary. Whilst the diarist is slightly less informative in the later years of his record, this does not explain the obvious difference between the relatively frequent observations in the years of the diary before 1751 and those after, when no cases of aurora are reported at all.

6. Conclusion

Brooks (1930) has expressed the importance of the study of the climate of the first half of the eighteenth century. On these grounds alone such a full record as Bulkeley provides of this period is a valuable source of confirmatory and additional data. Its value is increased by the fact that it deals with a locality on the west coast of Britain for which little early information is available. Whilst the agreement between the impressions gained from Bulkeley's diary and the reconstructions, which have been made for the same period, of the climate of other parts of the British Isles is usually a close one, there are sufficient differences and discrepancies to underline the potential regional climatic contrasts even within an area the size of Britain.

TABLE 5. BULKELEY'S RECORD OF AURORAE

Year (New Style calendar)	Number of occasions recorded	Date (night of)
1734 (from 11 Apr.)	1	28 Dec.
1735	1	15 Oct.
1736	3	25, 26 Oct, 7 Dec.
1737	0	—
1738	1	7 Apr.
1739	4	29 Mar., 30, 31 Oct, 1 Nov.
1740	2	21 Jan, 13 Nov.
1741	1	2 Oct.
1742	1	26 Mar.
1743 (to 19 June)	1	19 Mar.
1744 to 1746	No record	—
1747 (from 12 Aug.) to 1749	0	—
1750	2	26 Feb., 19 Oct.
1751 to 1760 (to 28 Sept.)	0	—

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A direct measurement of infra-red radiation divergence to 160 mb

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Summary

A simple, economical balloon-borne net radiometer is described. The net radiation is essentially obtained from the difference of fourth-powers of the temperatures of the bottom and top radiating surfaces, and the internal conduction, which is proportional to the temperature difference between the bottom and top surfaces. The results of a measurement of net radiation to 160 mb are discussed.

1. Introduction

Until recently, only direct measurement of the principal atmospheric radiating component, water vapour, and application of simplified radiative transfer equations and radiation charts have provided a means of evaluating the net radiation, its divergence, and the associated radiative cooling of the atmosphere. In the past few years a few net radiation measurements have been made. Brewer and Houghton (1956) have measured net radiation by means of upward- and downward-facing radiometers on the top and bottom of an aircraft. Pohl (1956) has made six measurements by means of a net radiometer carried aloft by balloons. In related experiments Gergen (1957) has made more than 300 measurements of the radiative equilibrium temperature of a spherical black ball carried aloft by balloon.

Direct measurement of net radiation is to be preferred to calculations. The latter are based on many simplifying and poorly understood assumptions, and require measurement of all radiating components, not only at the levels in question, but at levels above and below. Relative humidity measurements with the conventional radiosonde element are feasible only for humidities above some minimum value which depends on temperature; they fail in extremely dry layers of the troposphere and fail consistently above 300 mb. The presence and distribution of particulate matter or water in solid form are difficult to establish, while their radiative properties are matters of conjecture.

This note describes a simple, inexpensive method of direct measurement of net radiation with a balloon-borne radiometer, and discusses the results of a preliminary measurement. Suomi and Kuhn (1958) have already briefly discussed the principle of the instrument. This is a simple extension of their radiometer for balloon-borne use.

2. Theory of the instrument

The following symbols will be employed :

$R_{l\downarrow}$ = Downward flux of long-wave radiation from sky and clouds.

$R_{l\uparrow}$ = Upward flux of long-wave radiation from atmosphere below, and earth's surface.

a = Absorptivity of blackened mylar surface (0·85).

L = Effective reflectivity of two polyethylene films (0·16).

C_i = Internal flux of heat from downward-facing to upward-facing blackened mylar surface due to conduction and convection.

C_b = External heat flux by conduction and convection to the bottom, i.e., flux of heat to the lower blackened mylar surface from the outer polyethylene surface below.

C_t = Corresponding external heat flux by conduction and convection to the top, i.e., flux of heat to the top blackened mylar surface from the outer polyethylene surface above.

T_a = Absolute air temperature.

T_t = Absolute temperature of top mylar.

T_b = Absolute temperature of bottom mylar.

σ = Stefan-Boltzmann constant.

k_i = Thermal conductivity of air at mean of T_t and T_b.

k_t = Thermal conductivity of air at mean of T_t and T_a.

k_b = Thermal conductivity of air at mean of T_b and T_a.

K = Constant which multiplies conduction terms (1·37).

D = Distance between bottom and top of aluminized mylar surfaces.

d = Distance between outer polyethylene film and blackened mylar surface.

E_b = Error in calculation of radiation absorbed at bottom due to neglect of absorptions and emissions of polyethylene films, adiabatic air temperature changes inside the radiometer, and other small effects.

E_t = Corresponding error in calculation of radiation absorbed at top, but including also any error due to radiation from the balloon.

E_n = Error in calculation of difference in radiation absorbed due to the above causes and approximations to thermal conductivities.

g = Acceleration of gravity.

c_p = Specific heat of air at constant pressure.

Δp = Pressure at lower surface of a layer minus pressure at its top.

R_{nU} = Net radiation at the upper surface of an atmospheric layer.

R_{nL} = Net radiation at the lower surface of an atmospheric layer.

λ = Product of effective specific heat and effective mass/cm^2 of the thermistor-mylar combination at the bottom or top, experimentally found from speed of response to be 5×10^{-3} cal/cm^2 deg.

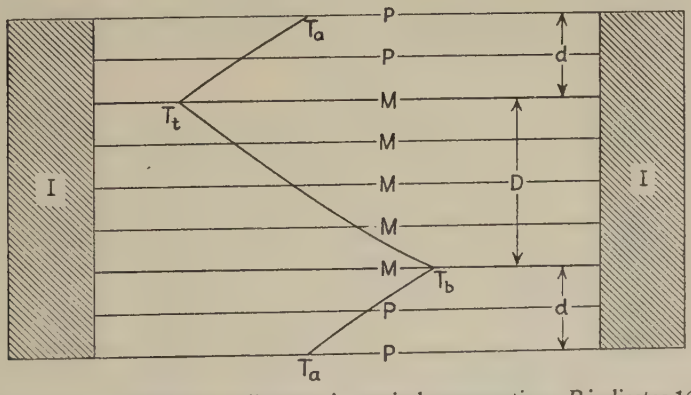

Figure 1. Schematic diagram of airborne radiometer in vertical cross-section. *P* indicates 12·7 μ polyethylene film. *M* indicates 6·4 μ mylar sheets whose inner surfaces are aluminized for reflection of long-wave radiation. Orientated qualitative temperature distribution within the radiometer for mid-troposphere conditions is given by zig-zag curve for which the temperature scale is horizontal, increasing to the right. *I* indicates styrofoam insulating material. Spaces between all surfaces are occupied by air at external pressure.

Fig. 1 shows a schematic diagram of the radiometer in vertical cross-section. Thermistors measure the temperatures of the uppermost and lowermost aluminized mylar surfaces, whose outward-facing surfaces are blackened. The instrument is attached to the side of the radiosonde or else held a short distance out from the radiosonde, the side of which is covered with reflecting aluminium foil. The thermistor temperatures are telemetered in a manner similar to that used to transmit air temperature in the ordinary radiosonde. A vertically orientated anemometer drives a sequencing switch in order to obtain air temperature, humidity, and top and bottom temperatures at regular intervals. The polyethylene films serve as convection shields and radiation windows. The vertical dimension ($D + 2d$) was 5·6 cm and the length of side of the triangular instrument was 30 cm.

For the derivation below suffice it to say that the polyethylene films have an infra-red transmittance of the order of 80 to 90 per cent at all wave-lengths; and of the 10 to 20 per cent loss in transmission, reflection accounts for roughly 90 per cent and absorption for roughly 10 per cent. Accordingly, the effects of absorption are neglected. A detailed discussion of the errors involved in this and other assumptions will be presented in Section 3, below.

When the instrument is orientated horizontally at night, the energy balance of the upper surface is approximately

$$a(1-L)R_{l\downarrow} + (1-a)(1-L)LaR_{l\downarrow} - a(1-aL)\sigma T_t^4 + C_i + C_t = \lambda\frac{dT_t}{dt} + E_t \quad . \quad (1)$$

Correspondingly, the energy balance for the lower surface is approximately

$$a(1-L)R_{l\uparrow} + (1-a)(1-L)LaR_{l\uparrow} - a(1-aL)\sigma T_b^4 - C_i + C_b = \lambda\frac{dT_b}{dt} + E_b \quad . \quad (2)$$

The conduction and/or convection terms are defined as follows :

$$C_i = k_i\frac{T_b - T_t}{D}; \quad C_t = k_t\frac{T_a - T_t}{d}; \quad C_b = k_b\frac{T_a - T_b}{d} \quad . \quad . \quad (3)$$

Simplifying Eqs. (1) and (2) by means of the fact that $L \ll 1$, we may express the radiative fluxes as

$$R_{l\downarrow} = \sigma T_t^4 + K[-C_i - C_t + \lambda(dT_t/dt) + E_t] \quad . \quad . \quad . \quad (4)$$

and

$$R_{l\uparrow} = \sigma T_b^4 + K[C_i - C_b + \lambda(dT_b/dt) + E_b] \quad . \quad . \quad . \quad (5)$$

where $K = [a\ (1-aL)]^{-1}$. Eqs. (4) and (5) allow the downward and upward radiation fluxes $R_{l\downarrow}$ and $R_{l\uparrow}$ to be computed. If Eq. (4) is subtracted from Eq. (5), the net radiation is obtained :

$$R_{l\uparrow} - R_{l\downarrow} = R_n = \sigma(T_b^4 - T_t^4) + K\left[2C_i + C_t - C_b + \lambda\frac{d}{dt}(T_b - T_t) + E_b - E_t\right] \quad . \quad (6)$$

The interpretation of the coefficients k_i, k_t, and k_b in Eq. (3) depends on whether the heat transfer through the air layers is by conduction (molecular) only or by conduction and convection simultaneously. When temperature increases upward between two rigid horizontal surfaces the transfer is invariably by conduction alone. When temperature decreases upward, the transfer may be either by conduction alone, or by both conduction and convection, depending on the several relevant factors which make up the dimensionless Grashof number. De Graaf and Van Der Held (1953) have determined the critical value of the Grashof number which separates conduction from a combination of conduction and convection. Evaluation of the extremes of Grashof number to be expected for the 0·7 cm thick layers used in the radiometer showed that heat transfer is always by conduction alone. Hence k_i, k_t and k_b are thermal conductivities. They are slowly varying functions

of temperature, and k_i is determined from $(T_b + T_t)/2$, k_t by $(T_a + T_t)/2$ and k_b by $(T_a + T_b)/2$. As a good approximation, k_i may be used in all three cases. In addition, the radiometer is so constructed that $d = D/2$, and Eq. (6) simplifies to

$$R_n = \sigma (T_b^4 - T_t^4) + K [4 C_i + \lambda \frac{d}{dt} (T_b - T_t) + E_n], \quad . \quad . \quad (7)$$

where, as will be shown in Section 3, E_n is a very small quantity. The radiative cooling may be computed from divergence of the net flux; the mean cooling in a layer is

$$\frac{\partial T}{\partial t} = \frac{-g (R_{nU} - R_{nL})}{c_p \Delta p} \quad . \quad . \quad . \quad . \quad (8)$$

3. Calculation of reflectivity and errors

Fig. 2 shows the per cent of normal beam transmittance of the 12·7 μ thick polyethylene film used in this instrument as a function of wavelength, as determined by a beam spectrometer. Although the measurements terminate at 30 μ, it is possible to estimate the transmittance farther into the long-wave region with considerable confidence. An inspection of Fig. 2 shows that the absorption bands are harmonically related, and that as one proceeds from shorter to longer wave lengths the bands become less intense and have a broader base. This is a typical characteristic of polymers. We should, therefore, expect the next important absorption band in the far infra-red to be centered about 56 μ and to have a broad base and less intensity. The polyethylene has an absorption band near the 6·3 μ water vapour band; however, it is not centred on the H_2O band and is considerably narrower. There are no absorption bands near an ozone band at 9·6 μ and an N_2O band at 7·8 μ. The weak polyethylene absorption band at 13·8 μ no doubt is broad enough to interfere with the radiation exchange between radiometer and CO_2, which has an intense band at 14·7 μ. Despite this overlap, the transmittance averages about 80 per cent in this region. On the basis of the extrapolated polyethylene absorption out to 56 μ, only minor interference will occur in the broad water-vapour band from 15 to 50 μ. Thin polyethylene is thus an excellent convection shield which will not interfere seriously with radiation exchange with the constituents of the atmosphere.

Fig. 2 also shows that polyethylene reflectivity greatly exceeds absorption, the fact used in writing down the energy balances Eqs. (1) and (2) where effects of polyethylene absorption and emission are included in small error terms. Fig. 2 affords a method of calculating the effective reflectivity L and showing that the polyethylene absorption contribution to error terms is no larger than 2 to 3 per cent of the principal radiation and conduction terms.

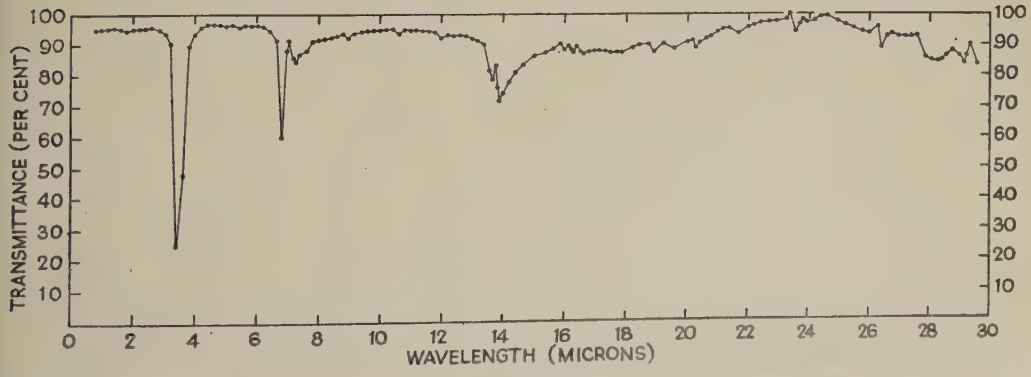

Figure 2. Per cent transmittance of 12·7 μ polyethylene film as a function of wavelength.

In order to calculate L the transmittance curve is smoothed by drawing straight lines across the top of the absorption bands. This new curve, which coincides with the original except over the absorption bands, represents the effects of reflectivity alone. The reflectivity was weighted according to black-body radiation at the particular wavelength, and for three black-body temperatures. Only a small variation of the percentage reflectivity occurred, as is expected from the uniformity of the reflectivity with wavelength. Next, the index of refraction of polyethylene (1·50) and Snell's and Fresnel's laws allowed a calculation of the reflectivity at other than a normal beam incidence. These reflectivities were weighted according to Lambert's law. From these calculations, a value of 0·16 was obtained for L. Since reflectivity is relatively uniform over wavelength, the same effective L will apply regardless of the fact that radiation received from water vapour and carbon dioxide is concentrated in certain spectral regions.

A value of L equal to 0·13 was obtained by a field experiment. In this experiment two additional layers of polyethylene were placed over the upper radiometer surface with the instrument 5 ft above the ground. A second balance equation was written for the upper surface, giving two simultaneous equations in $R_{l\downarrow}$ and L, from which $R_{l\downarrow}$ is eliminated and L calculated.

To calculate an effective absorptivity, it was necessary to consider the area beneath the curve representing transmittance with reflection alone, and the actual curve in Fig. 2 representing the transmittance with both reflection and absorption. The normal beam absorption weighted according to the Planckian distribution varied between 1·5 and 1·9 per cent for the temperature range from 220 to 290 K. No perceptible change of absorption occurred for angles of incidence up to 45°, the limiting angle of the spectrometer. The polyethylene emission to the upper and lower surfaces is therefore of the order of 2 per cent of a black body at the temperature of the polyethylene. If these terms were included in Eqs. (1) and (2), they would be of the order of 2 per cent of the terms representing loss of radiation from the blackened surfaces. Hence, these terms were neglected; alone they cannot make E_b and E_t more than 2 per cent of the largest terms. These emissions partially cancel in obtaining Eq. (7), and emission contributes to E_n an amount of the order of 2 per cent of the remaining terms.

The radiometer was suspended 75 ft below the balloon. Assuming the balloon is a black body, it contributes roughly a 1 per cent error to E_t.

Lateral heat conduction to the centre of the blackened surface contributes roughly 4 per cent of the terms retained.

The thickness of polyethylene can vary by 25 per cent over the whole film, and the separation of mylar and polyethylene surfaces cannot be controlled to better than a few per cent. Hence the calculation of radiative flux and divergence is subject to errors of a few per cent purely from the standpoint of controlling and measuring dimensions of the instrument.

A number of other effects could possibly lead to error, but experiments show that they do not. No frost or liquid water collects inside the radiometer as the air cools during the ascent, since ascents with a nitrogen-purged radiometer give the same results. Lateral drainage of air within the radiometer could presumably occur during swinging of the radiometer; however no perceptible change of temperatures was noted when the radiometer was swung through zenith angles up to 45° at ground level. Passing through a cloud the radiometer may collect a film of frost or water, but experiments near the ground show that motion of air past the radiometer evaporates any moisture within a minute or two. As the radiometer rises, roughly 90 per cent of the air within gradually drains out over a period of an hour. This outflow is uniform around the perimeter of the air layers; no internal circulation is established which effects a significant vertical heat transfer. Separate evidence that internal air motions are negligible was provided by a simultaneous ascent of two radiometers, one without the middle layer of polyethylene. The bottom temperature of the one-layer model was consistently about 1 C lower than in the two-layer model. The top temperature of the one-layer model was consistently about 1 C higher than in the two-layer model. The difference in temperature is therefore changed by only

2 C. The small decrease in temperature difference must reflect a small but negligible increase in the mobility of air within the instrument. The effect of adiabatic cooling of the air within the radiometer is to depress slightly the temperature inside the radiometer. This cooling is mostly compensated by conduction, as in the case of an ascending balloon.

At the ground the net radiation given by the air-borne radiometer was compared with that given by a precision ventilated radiometer. The comparison substantiated the value of K used in Eq. (7). Hence the analysis leading to Eq. (7) and the subsequent neglect of E_n are strongly supported at the ground.

To summarize, the various sources of error are emission of polyethylene, emission from the balloon, lateral heat conduction, uncertainties of polyethylene thickness and radiometer dimensions, collection of frost or liquid within the radiosonde, lateral air drainage due to swinging of radiometer, collection of moisture on external surfaces, lateral air drift due to seeping out of air and adiabatic cooling of air within the radiometer. In proceeding from the radiative fluxes to net radiation, the emission of polyethylene and the adiabatic cooling tend to be self-cancelling since the effects are the same below as above, while lateral heat conduction and lateral air drainage are self-adding. In view of all these various effects, the error terms in Eqs. (1), (2) and (7) are estimated to be no more than 10 per cent of the largest terms and are therefore neglected in the calculations. All errors act to lessen temperature differences. Accordingly, the downward flux of radiation is overestimated, the upward flux of radiation underestimated, and the net radiation underestimated.

In Eq. (7) the terms representing the difference in black-body radiation and the internal conduction are by far the largest and have roughly the same size.

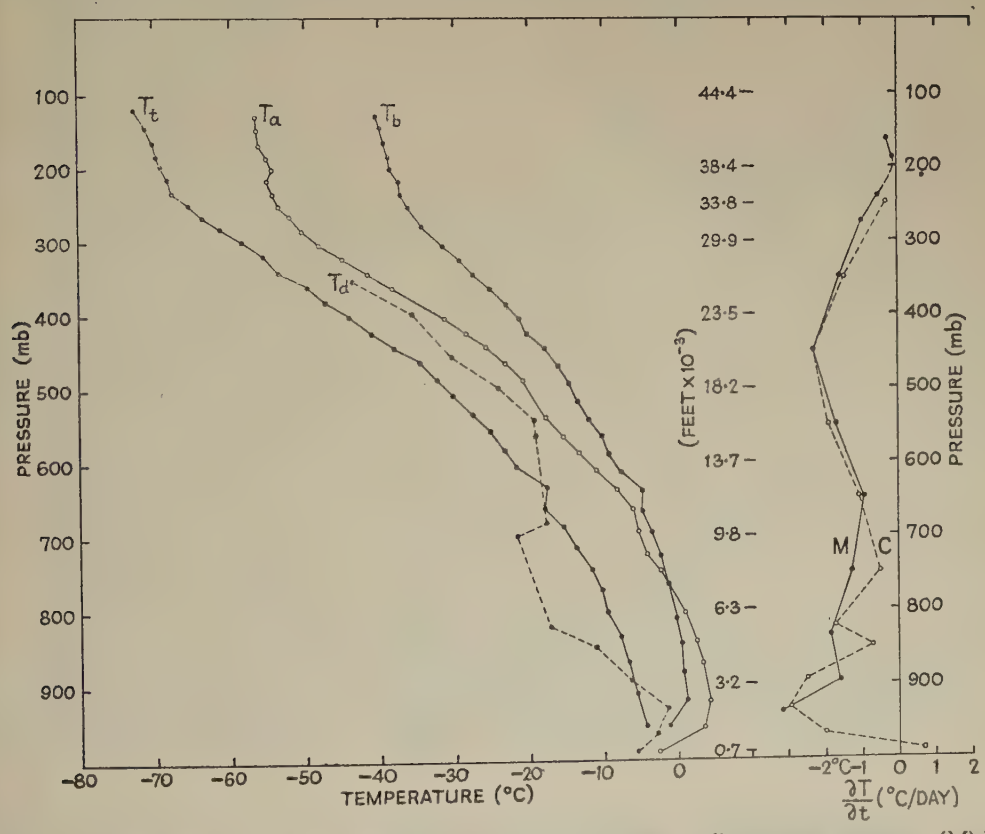

Figure 3. Air temperature (T_a), dew point (T_d), top (T_t) and bottom (T_b) radiometer temperatures. (M) is the radiative cooling measured from the vertical divergence of net radiation as given by Eq. (5). (C) is the radiative cooling calculated by Brooks's (1950) tabular method.

4. Results

The radiometer described above was sent aloft at Green Bay, Wisconsin, U.S.A. on 21 December 1956 approximately one hour before the regular 0300 G.M.T. sounding and well after dark. Fig. 3 shows air temperature, dew point as obtained from the more detailed regular 0300 G.M.T. sounding, and top and bottom radiometer temperatures. The right hand portion of Fig. 3 shows radiative cooling calculated from the height divergence of the net radiation given by Eq. (7), and also as given by Brooks's (1950) tabular method for solving Bruinenberg's (1946) equations applied to the more detailed regular sounding.

The temperature curve shows a surface inversion, two minor bumps in the troposphere, and a somewhat rounded tropopause. Radiometer bottom temperature first increases gradually, crosses the air-temperature curve, then decreases almost monotonically with height but remains higher than air temperature.

Radiometer top temperature generally decreases monotonically at all heights and is always the lowest of the three. The lapse rates of both top and bottom decrease upon entering the stratosphere.

The cooling rates calculated by Brooks's tabular method agree in mid-troposphere with the measured rates as given by Eq. (8). In and near the low dew-point region around 750 mb the humidity element is out of range; therefore the calculation becomes uncertain, and the disagreement in this region is probably not significant. Both results show large cooling at the top of the surface inversion. The Brooks calculations were extended to 250 mb by extrapolating the dew curve from 355 mb to 250 mb and assuming a specific humidity of 0·05 g/kg from 250 mb to the top of the water-vapour atmosphere arbitrarily placed at 100 mb. Hence the final calculated cooling may be somewhat uncertain.

The cooling profiles through the surface inversion and through the tropopause are of particular interest to one of us (D.O.S.). Radiation acts to reduce the curvature of the temperature profile at the top of the inversion and at the tropopause. It is difficult to establish this at the tropopause from calculation because of the lack of reliable moisture data (Staley 1957), but those calculations which have been made show that warming at the tropopause relative to layers below and above appears to be a persistent phenomenon.

Radiative cooling of a layer of the atmosphere occurs where the difference between bottom and top radiative temperatures increases with height. Since these two temperatures are measured at different times which correspond to height intervals the order of 350 m, smaller-scale divergences of net radiation cannot be detected with confidence. Moreover, any errors in the measurement of T_t or T_b can lead to large errors in $\lambda d\,(T_b - T_t)/dt$. When only one T_b and one T_t are measured in a shallow inversion, and at different levels, the calculation of $d\,(T_b - T_t)/dt$ is subject to large error. Thus the distribution of cooling through the small temperature profile irregularities is in all likelihood related to the dead zone of the recorder and the accuracy with which the recorder chart may be read. Too few observations were available in the surface inversion, and somewhat thicker layers were chosen. In mid-troposphere, where only very small irregularities are present, layers of 100 mb thickness were used for the calculation of cooling.

5. Concluding remarks

A working drawing of this instrument is available upon writing to the Department of Meteorology of the University of Wisconsin, at Madison, Wisconsin. Fig. 4 illustrates the complete instrument as it appears ready for flight. In later models an electric motor replaces the wind-driven switch.

We have described a relatively economical method of measuring the vertical distribution of long-wave radiation. Only very simple computations are necessary to proceed from measured radiometer temperatures to radiative flux and flux divergence. Many ascents have been made since the one described in this paper, and the experience strongly

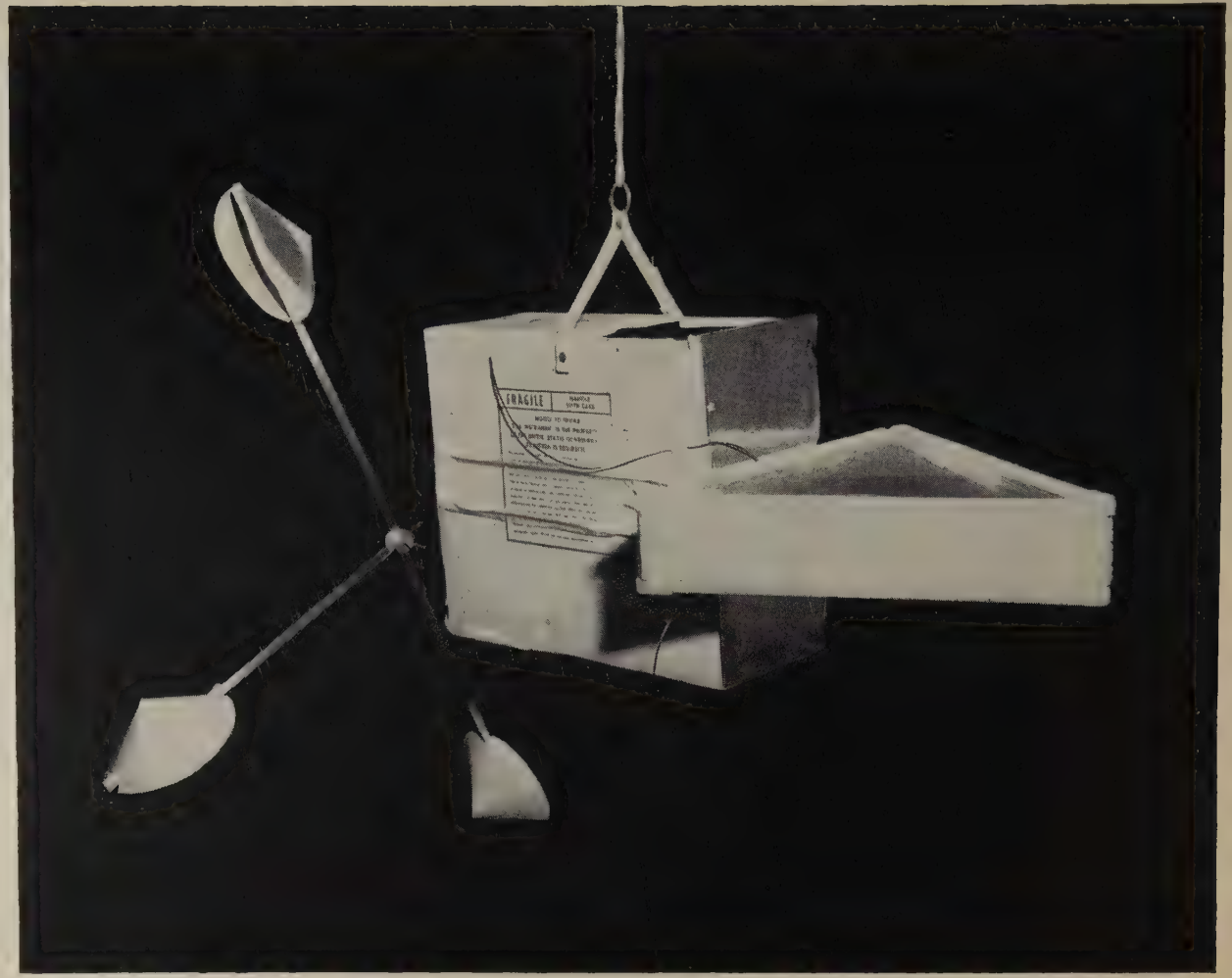

Figure 4. Modified radiosonde, 403 megacycle, U.S. Weather Bureau type, instrumented for radiometry.

To face page 140

suggests that measurements of radiation may be made on a routine basis at a network of stations. From such measurements the three-dimensional field of differential radiative heating would become available with regularity comparable to that of the more familiar fields of temperature and pressure. The implications of this for hydrostatic stability, baroclinic stability, general circulation and other studies are obvious.

Acknowledgments

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The meteorology of North Greenland during the midsummer period

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SUMMARY

An elevated ice sheet of the size of Greenland has a strong influence on its own weather. On account of the cooling by radiation of the snow surface and of the lower layers of the air there is a tendency for a system of low-level outflowing katabatic winds to be set up. The weather is also influenced by the cyclonic systems characteristic of middle latitudes and their associated fronts.

Northern Greenland is far from the main track of Atlantic depressions, and as the inland ice is here at its widest, conditions appear suitable for the establishment of the glacial anticyclone which would be expected to be disturbed only rarely by cyclonic systems. In order to study the frequency of these disturbances, and the normal meteorological conditions in the undisturbed periods, an investigation has been made of the weather in the area during the six midsummer weeks of 1953, in particular of the observations made at "Northice" * near the crest of the ridge of the inland ice and at Britannia Sø near the east coast.

Owing to the elevation of the ice sheet mean sea-level synoptic charts cannot be used satisfactorily, and a method of drawing 700 mb charts and using the same techniques as are used in temperate regions for analysis of mean sea-level charts has been developed. As the weather is predominantly fine all occurrences of appreciable cloudiness, precipitation and abnormal surface wind are significant and in almost all cases were due to cyclonic disturbances and their associated fronts which could be followed on the series of 700 mb charts. A total of 16 depressions or waves caused 19 disturbances (in three cases the effects of the warm and cold fronts were clearly separated) at either one or both of the two stations, but only 5 of these caused precipitation exceeding 1·0 mm at either station.

During the anticyclonic period of 11 days when the 700 mb wind was light and variable the weather at Northice was fine with a katabatic surface wind of 280° 13 kt. The diurnal range of temperature at Northice was 6·8°C but only 3·7°C at Britannia Sø. The diurnal variation of relative humidity was small. The upper winds at Northice were mainly S to W at low levels and S to E at 16 km.

A comparison of June 1953 with June 1954, and with statistics for other Greenland stations in other years, indicates that the midsummer of 1953 can probably be regarded as representative of a normal midsummer.

1. INTRODUCTION

Meteorological observations were made from November 1952 to July 1954 at the two stations set up by the British North Greenland Expedition at Britannia Sø (77° 09′ N, 23° 26′ W, altitude 229 m) in Dronning Louise Land, and at Northice (78° 04′ N, 38° 29′ W, altitude 2343 m) on the inland ice. The former station was on the north shore of a lake, Britannia Sø, in northern Dronning Louise Land, a mountainous land cut off from the coastal area by glaciers 30 km across. Britannia Sø is approximately 140 km west of the meteorological station at Danmarkshavn on the east coast of Greenland. Northice is 300 km from the nearest land in western Dronning Louise Land : the slope of the ice sheet is imperceptible though the ridge is 110 km further west.†

The observations made at Northice have been published in ful l(Hamilton and Rollitt 1957), while the Climatological Tables (Hamilton and Rollitt 1957) contain descriptions of the sites of both the stations, and of the instruments, and a statistical analysis of the observations. In this paper the meteorology of N. Greenland is studied in relation to the general synoptic situation.

The weather in Greenland was thought by Hobbs (1945) to be mainly controlled by the glacial anticyclone, but Dorsey (1945) has shown that in southern Greenland the weather on the inland ice is considerably affected by cyclonic disturbances and their associated fronts.

In northern Greenland where the ice sheet is more extensive and the area is further distant from the main track of Atlantic depressions it might be expected that the tendency for the glacial anticyclone to develop would be more marked : the greater extent of the snow surface losing heat by radiation would cause the outflow of katabatic winds over a

* "Northice" is not an accepted place name but is used in this paper for convenience.

† Maps of the area are published in *Geogr. J.*, **122**, 1956.

large area, the air being replaced by subsiding air from aloft. While this process certainly takes place it is clear from the observations made at Northice that the area is also considerably affected by fronts and depressions, and that the weather experienced is a result of the alternation between the influences of the anticyclonic and cyclonic systems.

2. Method of analysis

The technical difficulty which faces the meteorologist is how to draw conventional charts with fronts and pressure systems in this area where a large part of the inland ice is at an altitude of over 2,000 m. I have drawn 700 mb charts for this area, and have found that by drawing on them the fronts between the air masses and using the same techniques as are conventionally used in the analysis of m.s.l. charts, the changes from normal quiet weather can be explained by movements of fronts and depressions. The analysis is difficult compared with that of surface charts, as reports are neither so numerous nor so frequent, the temperature of air masses is changed by subsidence, and the humidity figures are frequently missing from the radiosonde reports, but in the main the movement of fronts and depressions can be followed without much difficulty at the 700 mb level.

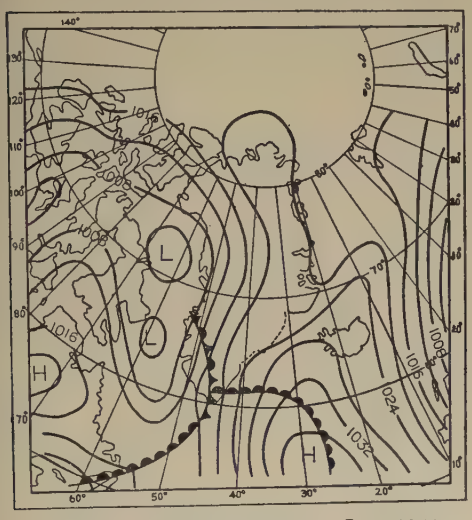

Figure 1. m.s.l. 1800 GMT 27 June 1954.

Figure 2. m.s.l. 1800 GMT 28 June 1954

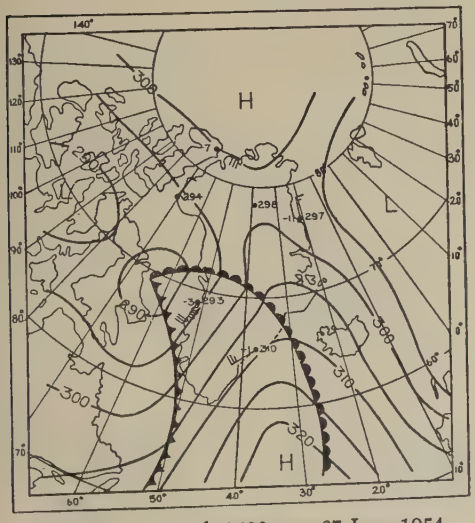

Figure 3. 700 mb 1500 GMT 27 June 1954.

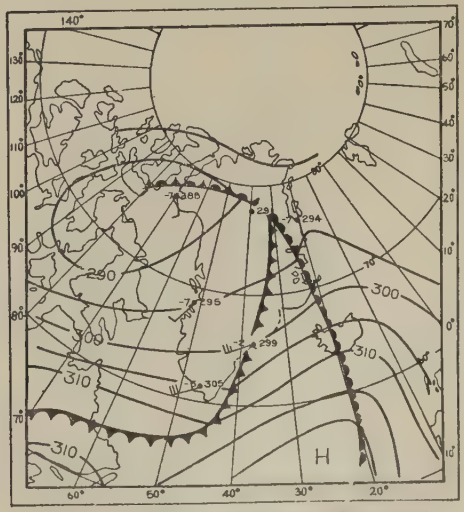

Figure 4. 700 mb 1500 GMT 28 June 1954.

The technique of analysis on the 700 mb chart will be illustrated below by example showing special weather conditions and sequences, but the surface and 700 mb charts for 27 and 28 June 1954 are reproduced here to illustrate the inadequacy of the analysis on m.s.l. charts and the advantage of using 700 mb charts.

The m.s.l. chart for 18 hr* 27 June (Fig. 1) shows a depression off SW Greenland with an occluded front drawn to the tip of the warm sector near Kap Farvel. The depression travelled along the SE coast of Greenland during the next 24 hours to its position in Fig. 2, and by 18 hr 29 June (chart not reproduced) had moved east of Iceland. It is difficult to understand from these charts why it was cloudy and light snow was falling at 12 hr on 28 June at Northice. The 700 mb chart for 15 hr 27 June (Fig. 3), however, shows the warm air spreading over southern Greenland. At Northice pressure began to fall after a maximum in the early morning and the westerly surface wind became light and backed to SSE. The 15 hr surface temperature was — 13°C. It was still almost cloudless at 00 hr 28 June but upper cloud increased and pressure fell rapidly during the morning to a minimum around 15 hr 28 June. Light snow was falling from 8/8 St at 200 m at 12 hr 28 June and ceased soon afterwards, and the wind became westerly again. The 15 hr surface temperature had risen to — 8°C. It is evident that a narrow warm sector or an occlusion passed over Northice around 12 hr 28 June. All changes in the meteorological elements which were experienced at Northice can be understood when followed on the 700 mb chart.

3. Synoptic types

A detailed study has been made of the weather experienced at Northice and at Britannia Sø during the period 1 June to 15 July 1953 in relation to the general synoptic situation in the Greenland area.

The Climatological Tables (Hamilton and Rollitt 1957) show that in the summer on the inland ice the weather is generally fine (less than 2/8 cloud) with westerly winds of 10 to 15 kt, but at times the sky is overcast, snow falls and there is a change of surface wind. These changes from normal fine weather are usually indications of frontal activity. At Britannia Sø, which is about 100 km from the coast, there is normally more cloud and the wind direction more variable, but changes from the normal weather are significant.

Analysis of the 700 mb charts of the area shows that during the period 1 June to 15 July 1953 as many as 19 depressions or frontal waves affected one or both of these stations. Details of these are given in Table 1.

(*a*) *Types of airflow*

As in temperate regions there are certain types of airflow which tend to persist for several days. During the 1953 summer these types were as follows :

1-6 June	W to SW type. Pressure was high SE of Greenland and depressions moved eastwards over N. Greenland (see Figs. 11 to 14).
7-9 June	Southerly type. Pressure was low over S. Greenland and high over Norway. An active depression moved northwards across Greenland (see Fig. 5).
9-12 June	Deep low over Canadian Archipelago : Cold S-Wly type with no fronts (see Fig. 6).
13-17 June	Southerly flow re-established as the deep low moved south.
18-30 June	E to SE type. Deep low SW of Iceland and anticyclone near Norway caused invasions of maritime air from N Atlantic (see Fig. 7).
30 June-10 July	Anticyclonic type. Norwegian high declined and lows moved on a more easterly track. High pressure developed over N. Greenland.
10-15 July	Cold S-Wly type (as 9-12 June).

4. Determination of altitude of 700 mb surface at Northice

The pressure readings at Northice have been used in the following manner in order to determine the altitude there of the 700 mb surface.

* All times, unless specifically stated otherwise, are in GMT.

A study of the radiosonde ascents made in 1949-50 (see, Expéditions Polaires Françaises 1950) at Station Centrale further south on the inland ice (70°N, 40°W, 2995 m) showed that in the summer months at 15 hr there was normally a temperature lapse in the lowest layers above the inland ice. When one exceptional case is omitted, the mean difference between the temperature at the surface and the temperature at the pressure level 50 mb less than station pressure was 4°C, with a standard deviation of 1·3°C. A few tethered-balloon ascents made at Northice confirm that the lapse rate at Northice was approximately the same. The estimated 700 mb temperature, T_{700}, is obtained from the measured screen temperature at 15 hr, T_{15}, by assuming that the lapse rate is 4°C per 50 mb. The mean temperature $\overline{T}$ of the layer between the station and the 700 mb surface is taken as $\frac{1}{2}(T + T_{700})$. From this the height of the 700 mb surface above barometer level is computed using the equation:

$$H = \frac{R}{Mg}\,\overline{T}\,(\log p - \log 700)$$

$$= 67{\cdot}4\,\overline{T}\,(\log p - \log 700) \text{ metres}$$

where p is the station level pressure and $\overline{T}$ is in °K. At Northice $g = 982{\cdot}2$ cm/sec^2.

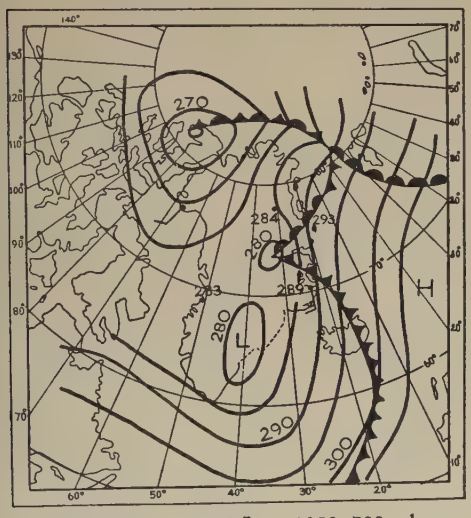

Figure 5. 0300 8 June 1953, 700 mb.

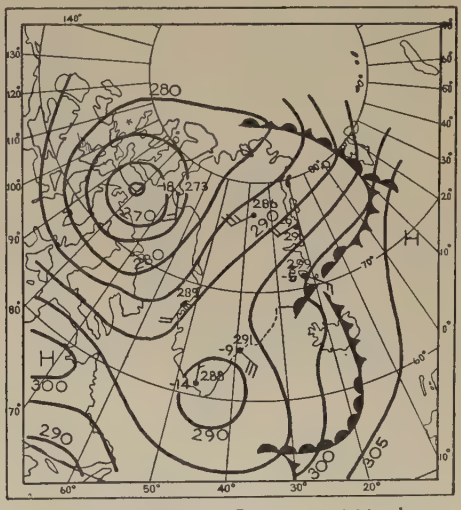

Figure 6. 1500 10 June 1953, 700 mb.

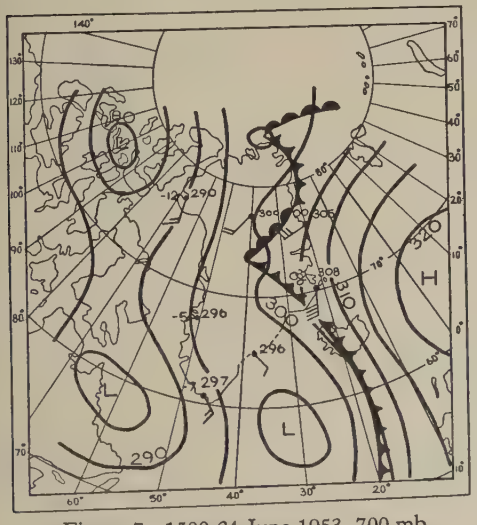

Figure 7. 1500 24 June 1953, 700 mb.

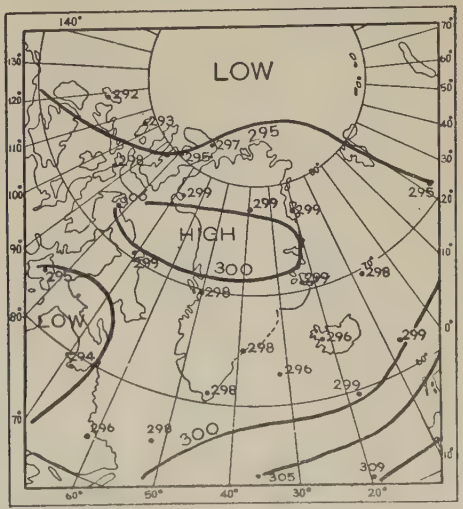

Figure 8. Mean, 30 June to 10 July 1953, 700 mb.

TABLE 1. WEATHER ASSOCIATED WITH PRESSURE SYSTEMS 1 JUNE TO 18 JULY 1953

		Northice			Britannia Sø		
June 1953	Frontal system at 700 mb ("N" = Northice B.S. = Britannia Sø)	Pressure minimum (GMT/date)	Precipitation (mm)	Weather (deg, kt, GMT)	Pressure minimum (GMT/date)	Precipitation (mm)	Weather (deg, kt, GMT)
1-2	Slow-moving occlusion, associated with low which moved eastwards across N. Greenland	05/2	0·5	Light snow 30/31, May, followed by two periods of snow on 2 June as centre passes over "N." 3,000 m wind 020/17 at 15	09/2	Tr.	Light snow afternoon 2nd
3	Wave moving eastwards passed S of "N" and over B.S.	13/3	Mod. to heavy	Int. snow morning with SE. surface wind. Became cont. snow, ceasing 1730. Wind backed through N to NW	21/3	Tr.	Light snow afternoon
4	Small cold low moved eastwards	21/4	Nil	3,000 m wind 180/17 at 15. No ppn. but 8/8 Cs 21	06/5	Nil	Fine
5	Warm front	—	Nil	8/8 Ac 14 and marked temp. rise	—	Nil	6/6 Cs at 21
5-6	Cold front	22/5 (flat)	Tr.	Light snow during night: remained cloudy until 17	06/6 (flat)	Nil	No cloud or ppn. but broken high cloud afternoon
8	Wave (with occluding fronts) moved northwards between "N" and B.S.	23/8	Nil	Southerly surface wind evening 7th, much cloud 8th, but no ppn, followed by strong NWly wind evening 8th and morning 9th	07/9	1·6	Cloudy, with rain and snow as warm front pased afternoon 8th, continued until passage of cold front 0630, 9th
13-14	Depression, with occluded fronts moved from SE almost over "N"	06/14	1·3	Snow afternoon 13th and wind E. On 14th, snow, then fine, then snow again. Wind light and variable	12/14 (very flat)	6·5	Rain morning 13th, becoming snow continuing until 14th: remained cloudy cleared later
17	Small depression with occlusion moved from S between "N" B.S. approx. 09/17	06/17	Nil	Fine, then cloudy with drift, clearing evening	12/17	Tr.	Cloudy with light rain early, clearing evening. Easterly wind
17-18	Vigorous ridge development 17th			Pressure rose 17 mb, 12/17 to 12/18			Pressure rose 19 mb, 18, 17th to 12, 18th
18	Warm front from SE	18/18	Nil	Bank of A-Cu passed over from S at 12. Rise in air mass temp. followed	—	Nil	Fine with 5/8 Ci Cu midday
20	Wave from SE passed over "N"	15/20 (flat)	0·1	Cloudy, with snow morning, cleared from SE	—	1·1	Cloudy morning, clearing evening

TABLE 1 (continued).

June 1953	Frontal system at 700 mb ("N" = Northice, B.S. = Britannia Sø)	Northice: Pressure minimum (GMT/date)	Northice: Precipitation (mm)	Northice: Weather (deg, kt, GMT)	Britannia Sø: Pressure minimum (GMT/date)	Britannia Sø: Precipitation (mm)	Britannia Sø: Weather (deg, kt, GMT)
22	Warm front of wave which passed S and W of "N"		0·3	Int. snow. Warm. Wind SE		0·8	Overcast with int. rain later
23	Cold front of wave	10/23	Nil	Cloudy, with mist and rime and ESE wind. Cloud broke midday, wind westerly. Fall in air mass temp. followed	20/23	Nil	Cloudy, clearing later
	Marked ridge development during 24th			Pressure rose 11 mb, 15/23 to 15/24			Pressure rose 18 mb, 21, 23rd to 21, 24th
25	Next wave passed E of "N"	07/25	Nil	Bank of A-Cu passed 07. 5/8 Cs midday otherwise fine. No ppn, wind light S became westerly	15/25 (flat)	Nil	Fine
26	Next warm front, accompanied by ridge development		Nil	Variable medium or high cloud		Tr.	Cloudy, with light rain afternoon 26th
27	Wave passed between "N" and B.S. about 09/27. Cold front passed B.S. 17/27	05/27	Nil	Fine	17/27 (flat)	Nil	Much cirrus at first becoming fine. Onset of N-Wly wind with A-Cu at 17. No ppn.
29	Next wave passed northwards over B.S.	Flat min. afternoon	Nil	5/8 Cirrus morning; then fine and calm	Rise began 18/29	Tr.	Much cloud; light rain 1630
July 1953							
1-10	High-pressure belt at 700 mb over Greenland			Fine except for fog followed by light snow 4th			Fine
10-11	Weak depression, with occluded front came from SW Greenland and passed "N" and B.S.	03/11 (slight)	Nil	Cirrus cloud	19/11 (slight)	Nil	Cirrus cloud
13	Front moving eastwards	12/13	Tr.	Increasing cloud, light snow evening	Rise began 20/13	Nil	Ci evening with N-Wly wind. No ppn.
15-18	Cold low passed S of "N"	Marked rise began 06/18	1·1	Strong westerly wind midday 15th became easterly with snow 18/15 and continued intermittently until 18th	Marked rise began 18/18	Tr.	Cloudy period 15th-18th, mainly easterly; slight ppn. 16th (soft hail 2215) and 18th 00-12

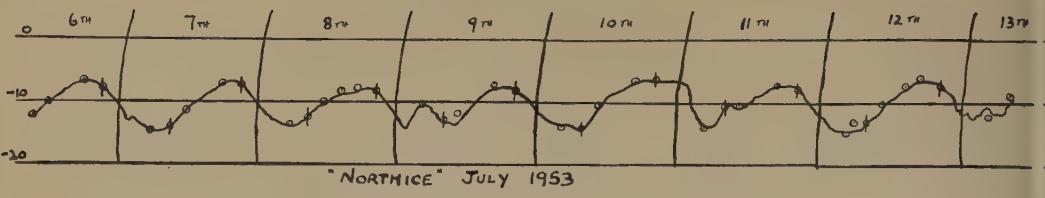

Figure 9

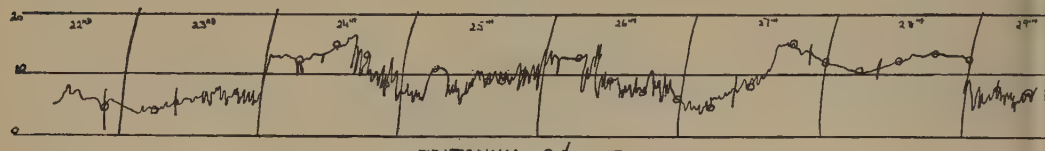

Figure 10

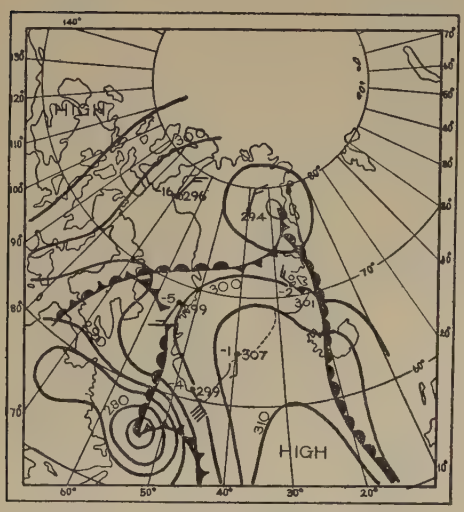

Figure 11. 1500 2 June 1953, 700 mb.

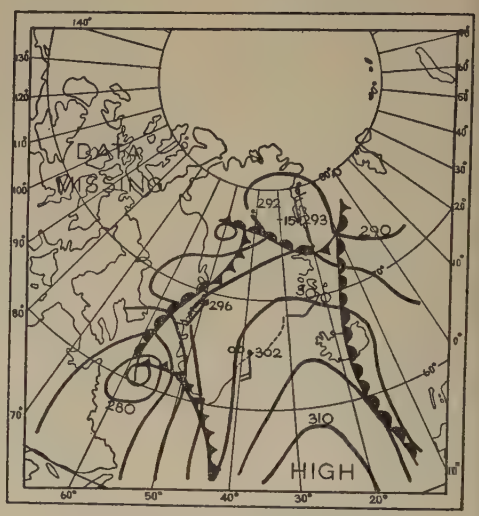

Figure 12. 0300 3 June 1953, 700 mb.

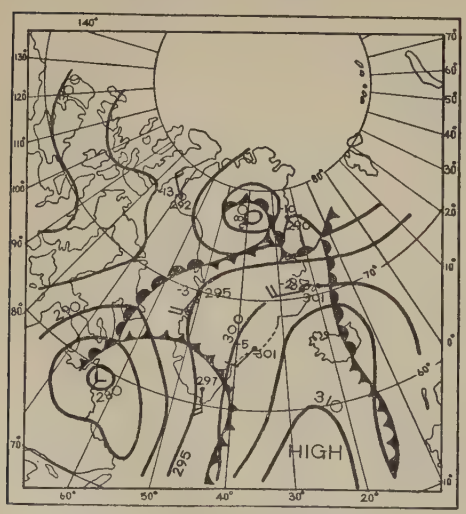

Figure 13. 1500 3 June 1953, 700 mb.

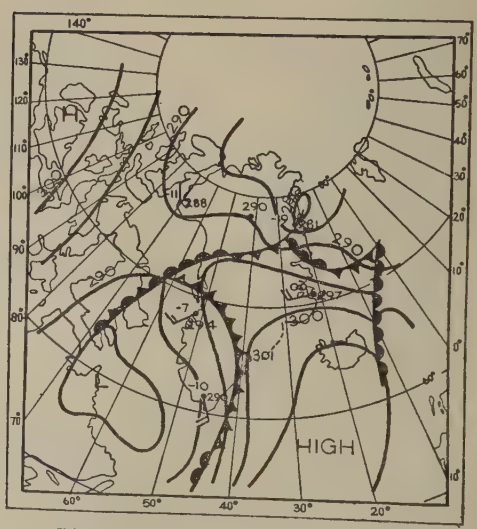

Figure 14. 0300 4 June 1953, 700 mb.

T_{700} is estimated daily at 15 hr from T_{15} (it is almost always $T_{15} - 4$). On occasions when T seems abnormal, a correction is made to the value of T_{700} obtained from the estimated lapse rate, based on the values for the previous and following days, and in the light of the synoptic situation. The value of T_{700} at 03 hr is estimated from the previous and following 15 hr values. The variation of T_{700} from day to day was small and exceeded 2°C only when there was a marked change of air mass.

It has been assumed that $\overline{T} = \frac{1}{2}(T + T_{700})$; this may not be strictly correct, but an error in $\overline{T}$ is approximately only one half the error in T_{700}, and leads to only quite a small error in ΔH. An error of 1°C in $\overline{T}$ introduces an error of only 2 m in ΔH.

The altitude of the barometer was measured by geodetic methods to be 2,343 m with a standard error of 10 m; this is equivalent to 2,350 geopotential metres.

As errors in ΔH exceeding 5 m, due to incorrect estimate of $\overline{T}$, are unlikely it may be taken that the error of H_{700}, the altitude of the 700 mb surface is about 10 m.

A table has been constructed which enables H_{700}, to be read off for any value of station pressure and estimated value of $\overline{T}$. This value, H_{700}, is plotted on the 700 mb charts. When pilot-balloon ascents have been made the 3,000 m wind is accepted as the 700 mb wind and plotted on the charts.

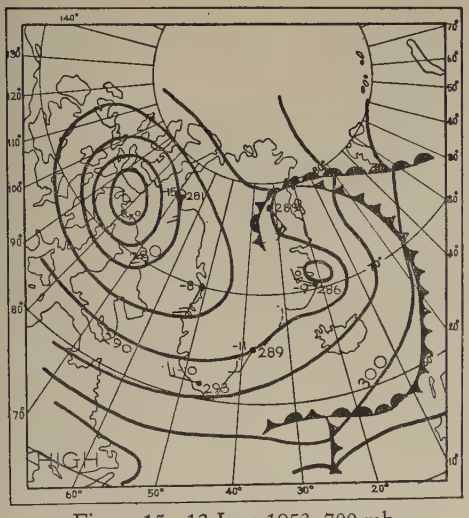

Figure 15. 13 June 1953, 700 mb.

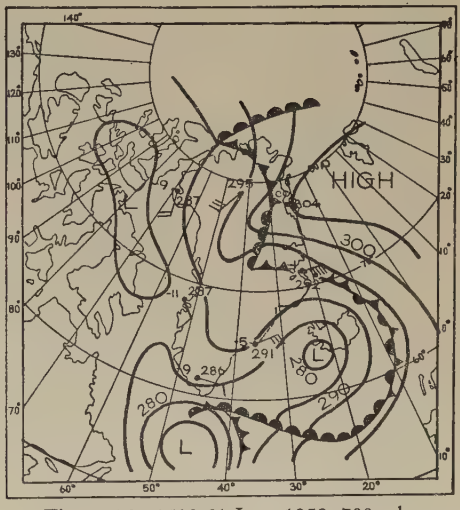

Figure 16. 1500 21 June 1953, 700 mb.

Figure 17. 1500 22 June 1953, 700 mb.

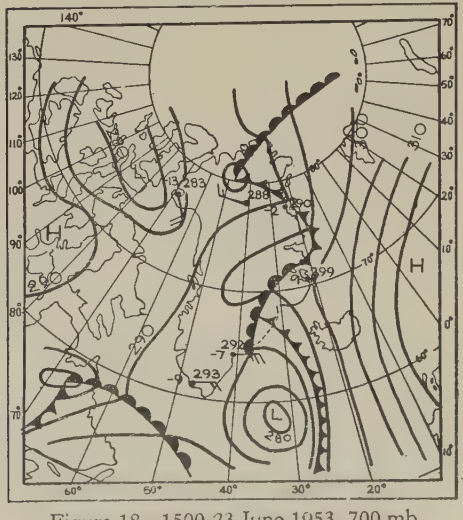

Figure 18. 1500 23 June 1953, 700 mb.

The mean 700 mb contour chart for the anticyclonic period 30 June-10 July 1953 (Fig. 8) shows that the mean value 2,990 m for Northice fits well with the values for neighbouring stations.

5. Synoptic situations of special interest

The periods of bad weather experienced at Northice during the period 1 June-15 July 1953 were as follows :

(a) 2-4 June (Figs. 11, 12, 13, 14). During 2 June a small centre passed over Northice and the fine weather of 1st became generally cloudy with light snow during the morning and early afternoon of 2nd. A wave off the west coast of Greenland deepened during the night of 2nd to 3rd and moved across N. Greenland south of Northice where easterly winds were experienced, the highest being 070° 30 kt; thereafter the pressure rose rapidly, and cloud cleared in the evening as the wind backed to the NW. At Britannia Sø there was light snow in the afternoon of 3rd with an easterly wind, ceasing with N-Wly wind at 16 hr on 3rd.

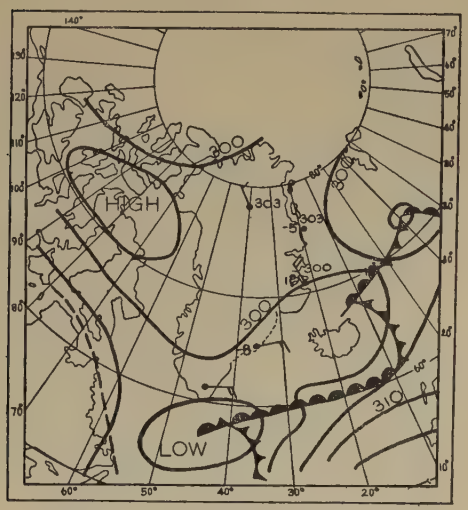

Figure 19. 1500 3 July 1953, 700 mb.

Figure 20. 1500 4 July 1953, 700 mb.

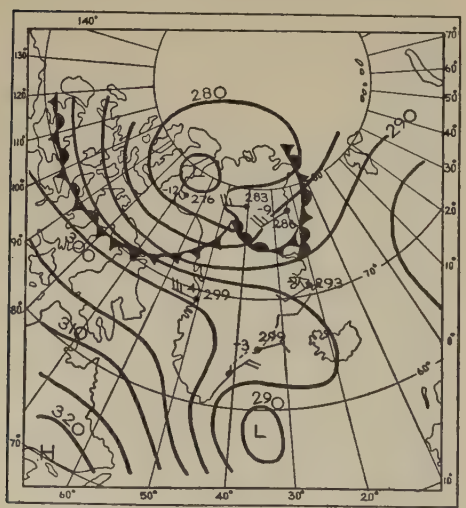

Figure 21. 1500 14 July 1953, 700 mb.

Figure 22. 1500 15 July 1953, 700 mb.

(b) 13-14 June (Fig. 15). Pressure was low over Canadian Archipelago and a depression with occluded fronts moved from the SE almost over Northice and snow fell there at intervals on 13 and 14 June, total 0·2 mm. At Britannia Sø there was rain on the morning of 13th which changed to snow and continued until the morning of 14th.

(c) 21-23 June (Figs. 16, 17 and 18). Pressure was high over Svalbard and waves moved northwards over eastern Greenland. The weather was fine and normal with 700 mb wind 230° 29 kt at 15 hr 21st at Northice, but strong southerly winds aloft, e.g. 190° 73 kt at 9,000 m – an unusually strong wind. On 22nd as the wave moved northwards the weather was cloudy and calm at Britannia Sø with intermittent rain late in the day.† In the Northice register the remark ' cloud moving rapidly from south ' was made at 06 hr and ' snow began 07 hr.' The warm front passed around 09 hr with light snow and high temperature and vapour pressure. At 15 hr the 700 mb wind at Northice was 160° 27 kt. On 23rd pressure fell at Britannia Sø and it remained calm and cloudy. At Northice the remark ' heavy rime on aerials; mist approaching from S ' was made at 09 hr and a fog bow (indicating water droplets) was seen at 10 hr. This was the cold front (pressure minimum at 10 hr) and by 12 hr the surface wind was 310° 8 kt and the cloud had broken. At 15 hr the 700 mb wind was 300° 20 kt but at 8,000 m it was 130° 23 kt.

(d) It is difficult to understand why fog occurred at Northice on the afternoon of 4 July. There was a ridge of high pressure over N. Greenland (Figs. 19 and 20) and 3 July was a normal cloudless day with 700 mb wind 340° 5 kt at 15 hr, and light variable winds above. The weather remained normal and cloudless up to 12 hr 4th, but ' cloud came down suddenly at 1330 hr. Glazed ice formed on thermometers in the screen.' At 15 hr there was fog with a surface wind of 040° 3 kt, at 18 hr fog with 180° 4 kt. Very slight snow fell just after 21 hr when it was calm. The wind was back to normal next morning and there was variable Sc at about 1,000 m above station level which dispersed in the evening. Though no front can be traced it seems possible that warmer air came round the high cell to the east of Northice. Weak fronts in anticyclonic situations in the polar regions are commonly associated with fog belts.

(e) Fog also occurred at Northice early in the morning of 9 July in a ridge of high pressure; again there is no indication of frontal activity. In this case it may have been a radiation fog which formed at night when the wind, for some reason, fell. It cleared by 08 hr, and a fine day with normal winds followed.

(f) Another period of appreciable snowfall at Northice occurred on 15 July (Fig. 22). There was variable cloud and the westerly wind was rather stronger than usual during the day as a cold low, moving eastwards over N. Greenland, passed almost over Northice. Snow began at 18 hr as the wind veered to 030° 20 kt and continued all night (total 0·5 mm) with a similar wind. The cloud cleared during the afternoon but the wind remained light easterly and a low temperature minimum (– 21°C) for the season was experienced. There was another light fall of snow with an easterly wind 12 hr to 18 hr on 17 July (presumably another cold low) and on 18th the wind was back to normal, though the cloud did not clear properly until the evening of 19th.

6. Normal weather during anticyclonic period

A study has been made of the undisturbed weather at Northice and Britannia Sø during the anticyclonic period 30 June-10 July 1953.

(*a*) *Northice*

(i) *Wind.* The surface winds at Northice at the synoptic hours shown at the head of the column were as given in Table 2 (in tens of degrees and knots). The last column gives the 700 mb wind at 15 hr. There is no appreciable diurnal variation of wind speed; the mean values (omitting the exceptional values, marked by *, during or just after a period of fog) indicate a maximum at 09 hr, but this seems to be of no statistical significance.

† At Myggbukta (73N, 22W), further south on the east coast, the rainfall for 22nd was as much as 38 mm.

Nor is there any apparent significant diurnal variation of direction. Of the 50 observations (omitting those marked *) 45 are in the range 260° to 300°, 10 to 16 kt. It appears that the normal midsummer surface wind is a shallow wind of topographic origin, which occurs when winds are light at 700 mb and is almost independent of the large-scale pressure distribution provided this is weak. Its value is about 280° 13 kt.

TABLE 2. SURFACE WIND AT NORTHICE

GMT	06		09		12		18		21		700 mb 15 hr	
	deg	kt	deg	kt	deg	kt	deg	kt	deg	kt	deg	kt
June												
30	280	11	300	15	300	15	300	13	270	10	180	05
July												
1	240	14	260	16	270	14	270	12	260	10	220	11
2	270	12	270	14	280	14	310	11	290	11	250	05
3	300	14	300	15	300	13	300	13	290	12	350	05
4	300	13	300	13	310	13	040	03*	Calm*		–	
5	290	09	300	10	300	09	280	13	280	11	310	03
6	270	16	280	15	290	15	280	15	290	15	290	09
7	280	16	280	18	280	13	280	16	290	13	290	05
8	280	16	280	16	280	14	280	12	270	11	230	09
9	270	02*	270	01*	240	08*	250	12	260	13	220	12
10	270	13	280	14	270	10	290	11	300	16	230	11
Mean speed	13·1		14·6		13·0		12·8		12·2			

(ii) *Cloud.* Except for the period of fog and stratus on 4th and 5th and again for a brief period on 9th, the cloud amount was never more than 1/8, generally a trace of Ci or A-Cu.

(iii) *Visibility* was almost always estimated to be at least 20 km except during the period of fog.

(*b*) *Britannia Sø*

(i) *Wind.* The surface winds observed at Britannia Sø are given in Table 3. There appears to be no marked diurnal variation of wind. This station is near sea level, and the pressure gradient at m.s.l. may not have been as flat as at 700 mb.

TABLE 3. SURFACE WIND AT BRITANNIA SØ

GMT	00		06		12		15		18		21	
July	deg	kt	deg	kt	deg	kt	deg	kt	deg	kt	deg	kt
1	260	02	340	22	090	13	100	18	100	21	110	18
2	120	10	100	08	120	12	110	14	110	19	110	11
3	090	02	330	15	320	16	280	11	070	02	300	15
4	310	13	320	15	310	20	310	20	360	17	010	13
5	010	19	110	12	260	01			100	15	220	05
6	270	06	Calm		310	16	290	09	310	16	310	17
7	300	18	330	14	330	20			310	16		
8			Calm		Calm		Calm		Calm		Calm	
9	Calm		Calm		Calm		Calm		Calm			
10	Calm		Calm		090	10			100	11		

(ii) *Cloud.* During the period 2-16 July there was normally no cloud or only a trace of medium or high cloud, but 2/8 fair-weather Cu was observed above the lake at 21 hr on 4 July.

(iii) *Visibility* was always 30 km or more.

7. Diurnal variation of temperature

(a) The mean air temperature at Northice for each hour was determined for the fine periods 15-22 June to 3-11 July 1953 from the dry-bulb temperatures and from the thermograph record. The curve of the mean temperature for each hour appears to be nearly sinusoidal with the equation

$$T = 261{\cdot}3 + 3{\cdot}5 \cos \frac{2\pi}{24} (t - 15{\cdot}0)$$

where T is in K° and t is measured in hours from midnight LMT. The differences between the temperatures determined by the equations and from the best curve drawn through the mean temperature for each hour are small, the maximum difference being 0·5° and the standard error 0·2°. The fall of temperature in the afternoon is slower than that given by the equation, but the second harmonic term will be small, and the equation above is accepted as expressing the diurnal variation of temperature during undisturbed weather at the midsummer period. The normal temperature range is 7·0° and the maximum occurs at 15 LMT (1730 GMT approx.). For the whole month of June 1953 the mean temperature was 260·6°K and the range 6·8°. Fig. 9 is a reproduction of the thermogram for the week 6-13 July 1953. The circles represent the screen temperatures.

(b) Analysis of the Britannia Sø thermograms for June 1953 shows that the diurnal range of temperature is 3·7°C, i.e. only about half the range at Northice: this may be due to the influence of the sea and on-shore winds, and to melting water on glaciers, lake, etc., in the neighbourhood. The time of maximum is around 1600 LMT, i.e. about 1730 GMT and the time of minimum about 0230 LMT, i.e. 0400 GMT. The sun will have been behind the hill to the north from about 2100 LMT to about 0400 LMT.

Fig. 10 is a reproduction of the thermogram for Britannia Sø for the week 22-29 June 1953. It illustrates how small is the diurnal variation of temperature and how variable is the temperature over periods of a few minutes when the wind is very light e.g. 12-21 hr on 23rd when the temperature fluctuated through a range of over 3°C or when it was moderate easterly, e.g. 17-21 hr on 26th.

In contrast the fluctuations are small when the wind is flowing off the inland ice, e.g. 02-14 hr on 24th and 18 hr 27th to 23 hr 28th.

Pronounced changes of temperature occur with wind changes, e.g. a rise of 8°C around 00 hr on 24th, 5°C around 15 hr on 27th on onset of the westerly wind from the inland ice, and a fall of about 6°C around 00 hr on 29th as the wind changed from westerly to easterly.

There were marked changes of water-vapour pressure on the three occasions just quoted – a fall of 6·7 to 4·5 mb and a fall from 7·1 to 4·5 mb on the first two occasions, and a rise of 5·2 to 6·1 mb on the last.

8. Humidity

(a) *Northice*

At Northice the relative humidity was recorded by a hair hygrograph, the readings of which were compared at intervals with determinations of relative humidity (with respect to water) made by a dry- and ice-bulb hygrometer. The standard error of the measurements of the hygrograph was 2 per cent.

There is very little variation of relative humidity, the mean diurnal curve for June 1953 showing a maximum of 80·5 per cent at about 0130 LMT (0400 GMT) and a minimum of 77·0 per cent about 1330 LMT (1600 GMT).

This slight diurnal variation of relative humidity is nearly 180° out of phase with the temperature variation, but as it is so small the variation of vapour pressure is in phase with the temperature variation with a minimum of 1·4 mb at about 0330 LMT (0600 GMT and a maximum of 2·4 mb at about 1630 LMT (1900 GMT).

The lack of variation of relative humidity is remarkable, but there seems to be no reason to doubt the readings of the hygrograph, the calibrations of which were satisfactory. The temperature was between — 20°C and 0°C, a range within which the hair hygrograph at Britannia Sø functioned well. The trace showed that the hygrograph responded to changes of humidity, i.e. it constantly fluctuated through a range of a few per cent, and indicated the passage of fronts. On two occasions in June and July 1953 of rime-depositing mist the indicated relative humidity with respect to ice was 96 per cent and on an occasion of fog a relative humidity with respect to ice of 100 per cent.

It appears, then, that mist and fog form when the air becomes saturated with respect to ice. It was found by Hamilton (1956) that in the winter at Britannia Sø rime was not deposited until the air was saturated with respect to water, but this was in calm clear conditions in the dark, with temperatures below — 25°C, and rime deposits may well occur at saturations with respect to ice on the inland ice during the summer when there is usually a moderate breeze and it is presumed there will always be small particles of drift snow present in the lower layers of the atmosphere.

The rise of vapour pressure in the afternoon will be due to increased sublimation from the snow surface, the temperature of which is presumably almost in phase with the air temperature. It is perhaps fortuitous that the diurnal variations of temperature and vapour pressure are such that there is almost no diurnal variation of relative humidity relative to water.

(*c*) *Britannia Sø*

At Britannia Sø the mean diurnal curve of relative humidity for June 1953 shows a maximum of 68 per cent at about 0230 LMT (0400 GMT) and a minimum of about 58 per cent at about 1730 LMT (1900 GMT). This is about 180° out of phase with the temperature variation and there is no appreciable variation of vapour pressure. Any small diurnal variation is masked by the marked change of relative humidity with change of wind. The relative humidity falls by 30-50 per cent on the onset of the westerly wind; on 9 June 1953 the relative humidity fell from 91 per cent at 06 hr to 18 per cent at 17 hr.

9. Upper winds

(*a*) *Northice*

Usually it was not possible to read the actual wind speed and direction on the wind dials down in the hut at the time of release of the balloon. The observer measured the azimuth and elevation of the balloon when it was at an estimated height of 60 m : in this way the mean wind in the lowest 60 m was determined (it is given as *H* in Table 4). This mean wind was not appreciably different from the estimated (occasionally measured) surface wind on release of balloon. The 2·5 km wind (150 m above surface) showed little change, and the 3 km wind (650 m above the surface) was the lowest which differed

TABLE 4. Upper winds – Northice 30 June-10 July 1953

GMT: Date	14 hr: 30		15 : 1		15 : 2		15 : 3		14 : 5		16 : 6		14 : 7		14 : 8		14 : 9		14 : 10	
Altitude	deg	kt	deg	kt	deg	kt	deg	kt	deg	kt	deg	kt	deg	kt	deg	kt	deg	kt	deg	kt
S	290	15	–		280	15	300	13	270	10	–		280	16	280	14	240	12	280	15
H	290	12	270	14	290	10	–		–		–		280	13	–		230	10	–	
2·5	280	10	270	12	310	11	310	11	290	11	300	16	260	12	270	13	240	12	280	19
3	180	05	220	11	250	05	350	05	310	03	290	09	290	05	230	09	220	12	230	11
5	110	06	150	05	180	05	040	05	190	02	250	05	310	10	210	15	210	19	180	16
7	040	07	100	10	190	12	210	09	–		280	04	350	12	190	11	220	18	160	19
10	070	10	110	11	160	18	090	01	–		340	15	330	30						
16			160	05	180	02	150	04												

Altitudes are in km above mean sea level. *S* is the surface wind.

TABLE 5. Upper winds – Northice

Altitude (above m.s.l.)	Velocity (kt)	Number of observations: Calm	N 340° to 020°	NE 030° to 060°	E 070° to 110°	SE 120° to 150°	S 160° to 200°	SW 210° to 240°	W 250° to 290°	NW 300° to 330°
	0-5		1				3	-	3	1
	6-15						2	7	9	1
3 km	16-25		2		1	1	4	8	13	3
	26-35						2	1	4	
	over 35								1	
	0-5		1	1		2	1	1	1	
	6-15				1		1	2	6	2
5 km	16-25						3	8	7	
	26-35						4		4	
	over 35					1	1			
	0-5				1					
	6-15		2		3		1		2	
10 km	16-25					1	3	2	1	1
	26-35						2		2	1
	over 35						3	1		
	0-5	2			2	2	2	1		
	6-15					1	4			
16 km	16-25									
	26-35									
	over 35						1			

TABEL 6. Upper winds – Northice (selected high ascents reaching 16 km)

Altitude (above m.s.l.)	Velocity (kt)	Number of observations: Calm	N 340° to 020°	NE 030° to 060°	E 070° to 110°	SE 120° to 150°	S 160° to 200°	SW 210° to 240°	W 250° to 290°	NW 300° to 330°
	0-5		1						2	
	6-15							3	2	
3 km.	16-25						1	3	2	
	26-35								1	
	over 35									
	0-5			1		1	1			
	6-15						1		1	
5 km	16-25							4	2	
	26-35						1		3	
	over 35									
	0-5				1					
	6-15		1		1		1		1	
10 km	16-25						3	1	1	
	26-35						2			
	over 35						1	1	1	
	0-5	2			2	2	2	1		
	6-15					1	4			
16 km	16-25									
	26-35									
	over 35						1			

appreciably from *H*. The winds at selected heights determined from all pilot-balloon ascents made at Northice during the special quiet period (30 June-10 July 1953) are shown in Table 4. The surface wind was generally around 280° 13 kt, and it can be seen that there was usually a fairly marked change of wind direction between 2·5 and 3·0 km and an appreciable change of wind speed : the mean wind speed at 3·0 km was 7·5 kt. It appears that the local effect of the ice sheet falls off between heights of 150 and 650 m above the surface. Except near the end of the period winds were generally light at all heights up to 20 km.

During the summer period June and July 1953 and 1954 wind frequencies were as shown in Table 5. It will be seen that the most common wind direction at 3 km (taken as the 700 mb wind) is W to SW and there is no appreciable change of pattern between 3 and 5 km. At 10 km (presumably often in the stratosphere) there is a definite tendency for the wind to back, south being now the favoured direction, and by 16 km the winds are generally lighter and more easterly.

That the backing and decrease of wind with height is a real effect is shown by Table 6 which is an analysis of those ascents which reached 16 km so that each ascent is represented in each part of the table. It will be seen that the general distribution of winds at the lower levels is much the same as in Table 5, and that in general the wind became lighter and backed with height.

The absence of strong winds in the summer months is noticeable; winds exceeding 35 kt were quite rare at all levels. On a few occasions where the wind speed exceeded 40 kt the wind was usually from the south. The strongest winds measured were on 21 June 1953 when a wind of 180° 80 kt at 10 km was measured. An unusual case was that of 16 July 1953 when easterly winds were observed to a height of 6 km. This was a synoptic situation of special interest.

(*b*) *Britannia Sø*

Not many pilot-balloon ascents were made at Britannia Sø during the summer. Those made between 6 July and 11 Aug. 1953 are given in Table 7. The ascents made when the surface wind was easterly showed a rapid decrease of the easterly component in the lowest kilometre, e.g. the ascents of 11, 14, 22 and 29 July and 2, 6, 7 and 11 August. When the surface wind is westerly the westerly airstream is probably a few kilometres deep, but usually the wind speed decreased with height. The surface wind, therefore, is considerably controlled by katabatic and anabatic effects.

10. Review of midsummer weather 1953

The foregoing pages show that during the midsummer period of 1953 the weather in the area of N. Greenland under consideration was generally fine, with a general pattern of local winds, and that the general pattern was broken down by travelling cyclonic systems and their associated fronts. Of the 19 disturbances which could be traced and which are listed in Table 1 :

5 caused precipitation exceeding 0·1 mm at Northice
3 caused 0·1 mm or less at Northice.
11 passed without causing any precipitation at Northice
4 caused precipitation exceeding 0·1 mm at Britannia Sø
6 caused 0·1 mm or less at Britannia Sø
9 passed without causing any precipitation at Britannia Sø

The only disturbances which caused appreciable precipitation at both stations are described in sections 5 (b) and 5 (c) : these approached from the SE. The amounts of precipitation were much larger at Britannia Sø than at Northice. All appreciable falls at Britannia Sø occurred in a S-ly or S-Ely type.

The other appreciable snowfalls at Northice, viz., those described in 5 (a) and 5 (b) occurred in a W to SW type : these caused inappreciable precipitation at Britannia Sø on the lee side of the inland ice.

TABLE 7. UPPER WINDS – BRITANNIA Sø

	July 1953						
GMT : Date	16 hr : 6	17 : 11	17 : 12	16 : 14	15 : 22	15 : 23	17 : 25
Altitude	deg kt	deg kt	deg kt	deg kt	deg kt	deg kt	deg kt
S	320 14	090 30	270 10	110 16	100 16	290 26	300 26
0·5	320 19	110 12	120 04	170 03	090 04	320 16	330 14
1	300 15	250 08	270 04	280 02	200 03	340 11	310 11
2	310 19	240 15	330 03		220 07		
3	310 12	210 24	230 08		240 02		
5	280 04	210 18	230 01		080 08		
7	210 10	220 08	140 03		080 09		

	July				August		
GMT : Date	15 : 29	19 : 30	15 : 1	16 : 2	15 : 3	15 : 4	03 : 5
Altitude	deg kt	deg kt	deg kt	deg kt	deg kt	deg kt	deg kt
S	–	310 25	220 06	090 12	290 18	300 13	320 30
0·5	110 02	300 28	110 03	090 09	300 25	290 19	310 32
1	330 06	280 13	Calm	Calm	310 21	290 15	330 26
2	270 05	290 10	220 15	270 06	310 32	290 30	330 25
3	250 02		260 12	210 18	250 10		
5				200 28			

	August					
GMT : Date	16 : 5	16 : 6	15 : 7	15 : 8	15 : 10	15 : 11
Altitude	deg kt	deg kt	deg kt	deg kt	deg kt	deg kt
S	–	110 12	110 14	–	090 14	080 13
0·5	280 13	130 06	130 05	060 20	120 10	100 13
1	310 23	310 03	220 05	120 03	230 02	130 05
2	300 32	300 17	260 18	160 11	220 08	
3	330 07	300 20	270 20		200 18	
5	290 27					

Altitudes are in km above mean sea level. *S* is the surface wind.

(a) *Comparison of June 1953 and June 1954*

This midsummer period of 1953 has been considered in some detail as the author was at Northice throughout the period. It is important to ascertain, if possible, whether this period was fairly normal and may be regarded as reasonably representative.

The following statistics for the months of June enable some comparison to be made between 1953 and 1954 :

	Northice		Britannia Sø	
	June 1953	June 1954	June 1953	June 1954
Mean cloud amount (eighths)	3·4	2·9	3·8	4·7
Mean low cloud amount (eighths)	1·7	1·7	1·6	3·2
Mean wind speed (kt)	11·0	9·2	11·0	12·6
Percentage of winds (210°-020°)	74	67	39	53
Percentage of winds (030°-200°)	20	28	39	43
Mean station-level pressure (mb)	754·1	755·7	983·7	986·7
Number of days of precipitation (a) 0·1 mm or more	5	7	4	3
Number of days of precipitation (b) 1·0 mm or more	3	2	3	2
Total precipitation mm	*	3·9	10·0	10·0

* Not measured

An analysis for June 1954 similar to that in Table 1 shows that during the month there were 13 disturbances which could be followed on the 700 mb charts and which affected Northice or Britannia Sø. The corresponding number in 1953 was 16, but whereas of these only 10 caused precipitation at one or both of the stations, in 1954 they all produced precipitation at one or both of the stations. In 1954, however, almost all the precipitation at Britannia Sø occurred on 19 and 20 June after passage of a warm front, when the cold front was held up by waves to the west, and a S-Ely stream of warm air from the North Atlantic caused almost continuous precipitation, mainly orographic, on the coast.

From this analysis of the charts, and the table showing comparisons for June 1953 and 1954, it appears that the weather in both these years was very similar at Northice and that there was not a great difference between the two years at Britannia Sø.

(*b*) *Comparison with other years at other stations*

The precipitation figures for June for stations in N. Greenland are as follows :

Station	Position	1953 mm	1954 mm	Mean mm	Years for Mean
Northice	78N 38W	–	3·9		
Britannia Sø	77N 24W	10	10		
Danmarkshavn	77N 19W	10	23	11	1949-54
Thule	76½N 67W	5	8	4	1946-54
Daneborg	74N 20W	34	31		
Myggbukta	73N 22W	86	36	19	1946-54
Upernavik	73N 56W	4	11	8	1951-54
Scoresbysund	70N 25W	106	35	29	1950-55
Jakobshavn	69N 51W	10	23	11	1949-54

It will be seen that at the stations farther south on the east coast the precipitation in June 1953 was above the average, but at the remaining stations the June 1953 figures do not differ very markedly from the mean values for June, and that June in 1954 was wetter than in 1953 at these coastal stations. It seems likely, therefore, that the weather at Northice and, perhaps to a less extent, the weather at Britannia Sø, as described in this paper, may be regarded as representative of the midsummer period in an average year.

Acknowledgments

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551.558.21

Tractable models of steady-state stratified flow with shear

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1. Introduction

In an earlier paper by the author (Long 1953) the primitive equations of a stratified frictionless liquid in two-dimensional, steady motion were integrated once to yield the non-linear, second order differential equation*

$$\nabla^2 y_0 + \tfrac{1}{2}\left[(\nabla y_0)^2 - 1\right] \frac{d}{dy_0}(\ln U^2 \rho) - \frac{g}{U^2 \rho}\frac{d\rho}{dy_0}(y_0 - y) = 0 \quad . \quad . \quad (1)$$

The vertical coordinate is y, the horizontal coordinate is x, and $y_0\,(x, y)$ is the height far upstream of the streamline passing through the point (x, y). The velocity and density distributions upstream are $U\,(y_0)$ and $\rho\,(y_0)$ and g is gravity. Eq. (1) reveals that for the case $U\rho^{\frac{1}{2}} = \text{constant}$, ρ linear with height,

$$\nabla^2 \delta + \alpha^2 \delta = 0, \quad \alpha^2 = \frac{g}{U^2}\left|\frac{1}{\rho}\frac{d\rho}{dy_0}\right| = \text{const.}, \quad \delta = y_0 - y \quad . \quad . \quad (2)$$

The model in Eq. (2) was investigated in detail by Long (1955). Solutions were derived for flow over obstacles of finite dimensions and compared with experiments. The experimental model had the distributions $U = \text{const.}$, and ρ decreasing smoothly and slowly in the vertical, but the phenomena were essentially the same as in the theory.

In most geophysical applications of stratified flow theory, the density varies very slowly in the vertical so that Eq. (2) will be the governing equation, provided (1) the density gradient is smooth, and (2) $U^2\rho$ is nearly constant. The second condition implies that U is nearly constant. This is inadequate for many problems such as air flow over mountains and hills in which the wind normally increases rapidly with height. The basic Eq. (1) is too complicated to use in the general case and it is of interest to search for tractable theoretical models of shearing currents. The purpose of this note is to present the results of this search.

2. Tractable models of shearing currents with density stratification

If we define the following :

$$f(x, y) = \int_0^{y_0} z\,(y_0)\,dy_0, \quad f_1 = \int_0^{y} z\,(y_0)\,dy_0, \quad f_0 = \int_y^{y_0} z\,(y_0)\,dy_0, \quad z = U\rho^{\frac{1}{2}} \quad . \quad (3)$$

Eq. (1) takes the form

$$\nabla^2 f + G(f, y) = 0, \quad G(f, y) = -\frac{dz}{dy_0} - \frac{g}{z}\frac{d\rho}{dy_0}(y_0 - y) \quad . \quad . \quad (4)$$

If $y \equiv y_0$, $f \equiv f_1$, there is no disturbance and f_1 must be a solution of (4), i.e.,

$$\nabla^2 f_1 + G(f_1, y) = 0 \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Subtracting (4) and (5)

$$\nabla^2 f_0 + G(f, y) - G(f_1, y) = 0 \quad . \quad . \quad . \quad . \quad (6)$$

or

$$\nabla^2 f_0 + \frac{\partial G}{\partial f} f_0 + \frac{\partial^2 G}{\partial f^2}\frac{f_0^2}{2!} + \ldots = 0 \quad . \quad . \quad . \quad (7)$$

where the partial derivative with respect to f represents differentiation of $G\,(f, y)$, holding y constant.

* A comparable result for a compressible fluid was also obtained by the author (Long 1956). The effect of compressibility is negligible if the vertical displacement of the fluid is not too large.

If we assume the special form

$$\frac{\partial G}{\partial f} \equiv \alpha^2 + \beta y \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

where α and β are constants, (7) becomes

$$\nabla^2 f_0 + (\alpha^2 + \beta y) f_0 = 0 \quad . \quad . \quad . \quad . \quad (9)$$

and the governing differential equation of any model contained in (8) is linear. Using the relation

$$\frac{\partial G}{\partial f} = \frac{1}{z}\frac{\partial G}{\partial y_0} \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

(8) becomes

$$\frac{1}{z}\frac{d}{dy_0}\left[-\frac{dz}{dy_0} - \frac{g}{z}\frac{d\rho}{dy_0}y_0\right] + \frac{y}{z}\frac{d}{dy_0}\left(\frac{g}{z}\frac{d\rho}{dy_0}\right) \equiv \alpha^2 + \beta y \quad . \quad . \quad (11)$$

or

$$\beta = \frac{1}{z}\frac{d}{dy_0}\left(\frac{g}{z}\frac{d\rho}{dy_0}\right), \qquad \alpha^2 z = \frac{d}{dy_0}\left[-\frac{dz}{dy_0} - \frac{g}{z}\frac{d\rho}{dy_0}y_0\right] \quad . \quad . \quad (12)$$

Integrating both equations in (12)

$$\beta f = \frac{g}{z}\frac{d\rho}{dy_0} + \gamma \quad . \quad . \quad . \quad . \quad . \quad (13)$$

$$\alpha^2 f = -\frac{d^2 f}{dy_0^2} - \beta y_0 f + \gamma y_0 + C \quad . \quad . \quad . \quad (14)$$

where γ and C are constants of integration.

The solution of Eq. (14) involves Bessel functions $J_{\frac{1}{3}}$ and $J_{-\frac{1}{3}}$. The result is

$$f = \frac{\pi}{\beta\sqrt{3}}(\alpha^2 + \beta y_0)^{\frac{1}{2}}\left\{J_{\frac{1}{3}}\left[\frac{2(\alpha + \beta y_0)^{3/2}}{3\beta}\right]\int (C + \gamma y_0)(\alpha^2 + \beta y_0)^{\frac{1}{2}} J_{-\frac{1}{3}}\left[\frac{2(\alpha + \beta y_0)^{3/2}}{3\beta}\right] dy_0\right.$$
$$\left. - J_{-\frac{1}{3}}\left[\frac{2(\alpha + \beta y_0)^{3/2}}{3\beta}\right]\int (C + \gamma y_0)(\alpha^2 + \beta y_0)^{\frac{1}{2}} J_{\frac{1}{3}}\left[\frac{2(\alpha + \beta y_0)^{3/2}}{3\beta}\right] dy_0\right\} \quad . \quad (15)$$

$$\frac{d\rho}{dy_0} = -\frac{\gamma U\rho^{\frac{1}{2}}}{g} + \frac{\beta U\rho^{\frac{1}{2}}}{g} f \quad . \quad . \quad . \quad . \quad (16)$$

Although these expressions are quite involved, they determine the basic velocity and density distributions after a choice of the constants α, β, γ, C and the two integration constants of the indefinite integrals.

3. Model for $\beta = 0$

If β is zero, it follows directly from Eqs. (13) and (14) that

$$U\rho^{\frac{1}{2}} = \frac{\gamma}{\alpha^2} + A\cos\alpha y_0 + B\sin\alpha y_0 \quad . \quad . \quad . \quad . \quad (17)$$

$$\frac{d\rho}{dy_0} = -\frac{\gamma}{g}\left(\frac{\gamma}{\alpha^2} + A\cos\alpha y_0 + B\sin\alpha y_0\right) \quad . \quad . \quad . \quad (18)$$

The differential equation, obtained from the definition of f_0, is

$$(\nabla^2 + \alpha^2)\left[y_0 - y + \frac{A\alpha}{\gamma}(\sin\alpha y_0 - \sin\alpha y) - \frac{B\alpha}{\gamma}(\cos\alpha y_0 - \cos\alpha y)\right] = 0 \quad . \quad (19)$$

The special case of $A = B = 0$ yields the model of Eq. (2).

Eq. (19) has a very simple form if we substitute $\epsilon = \rho(y_0) - \rho(y)$ as the dependent variable. Integrating Eq. (18) we find

$$(\nabla^2 + \alpha^2)\,\epsilon = 0. \qquad (20)$$

This has the same form as Eq. (2). In the case of flow in a channel with a rigid plane upper surface $y = H$ and an obstacle $y = f(x)$ in the vicinity of $x = 0$ at the bottom, solutions may be obtained in a simple manner. Suppose first that the obstacle has an infinitesimal height. Then $\epsilon = 0$ at the upper surface, and $\epsilon = \dfrac{\gamma^2}{\alpha^2 g}\left[1 + \dfrac{A\alpha^2}{\gamma}\right] f(x) + 0(f^3)$ at $y = f(x)$. Defining

$$-\delta^* = \epsilon \frac{\alpha^2 g}{\gamma^2} \Big/ \left(1 + \frac{A\alpha^2}{\gamma}\right) \qquad (21)$$

the problem is to solve the system

$$(\nabla^2 + \alpha^2)\,\delta^* = 0 \quad \left\{ \begin{array}{l} \delta^* = 0 \text{ at } y = H, \\ \delta^* = -f(x) \text{ at } y = f(x) \end{array} \right\} \qquad (22)$$

which is identical with the problem solved by Long (1955) for the model of Eq. (2). Thus for a given obstacle and a given constant, α,

$$\epsilon = -\frac{\gamma^2}{\alpha^2 g}\left(1 + \frac{A\alpha^2}{\gamma}\right)\delta^*$$

where δ^* may be considered a known function of (x, y)†. We now observe that δ^* is proportional to a, the maximum height of the obstacle $y = f(x)$. If we assign arbitrarily a finite value to a, the differential equation (20) is still satisfied, as is the condition $\epsilon = 0$ at $y = H$. The kinematic condition at the obstacle is no longer met, however, but we may choose as the new channel bottom $\rho(x, y) = \rho_0$, where ρ_0 is the value of the density far upstream at the channel bottom.

The above procedure solves for $\epsilon(x, y) = \rho(y_0) - \rho(y)$. The velocity field may be inferred from the lines $\rho = \text{const.}$, by noting that the direction of the flow is along $\rho = \text{const.}$, and that the speed is

$$c = \left|\frac{\partial \psi}{\partial n}\right| = \left|\frac{\partial \rho}{\partial n}\frac{d\psi}{d\rho}\right| = \frac{g}{\gamma \rho^{\frac{1}{2}}}\left|\frac{\partial \rho}{\partial n}\right|$$

where n is in the direction of the local density gradient. Since $\rho^{\frac{1}{2}}$ is nearly constant in most cases of interest, the speed is nearly proportional to the spacing of the lines $\rho = \text{const.}$

4. The general model of Eqs. (15) and (16).

Solution of the general model may be obtained along the lines of section (3) to obtain $f_0(x, y)$. The velocity field is inferred from the lines $f = f_0 + f_1$. The direction is along $f = \text{const.}$ and the speed is

$$c = \left|\frac{\partial \psi}{\partial n}\right| = \left|\frac{d\psi}{df}\frac{\partial f}{\partial n}\right| = \frac{1}{\rho^{\frac{1}{2}}}\left|\frac{\partial f}{\partial n}\right|$$

Again if ρ is slowly varying the speed is nearly proportional to the spacing of the f lines.

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† δ^* is given in the Appendix of a Technical Report (unpublished) by the author for a simple obstacle form of half-length b and maximum height a, for a number of values of a and b.

551.509.57 : 551.574.5

The decay of the ice-nucleating properties of silver iodide released from an aircraft

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(Manuscript received 4 November 1957)

SUMMARY

Silver-iodide smoke was released from an aircraft, together with a tracer of zinc sulphide, and both aerosols were detected in another aircraft. About 2×10^{14} freezing nuclei were emitted per gram of silver iodide burnt. The number of effective nuclei decreased on exposure to the air in the daytime by a factor of 1,000 in two hours. At night there was no detectable decay.

1. INTRODUCTION

In previous investigations (Smith, Heffernan and Seely 1955; Smith and Heffernan 1956) silver-iodide smoke, together with a tracer aerosol of zinc sulphide, was released from the ground and detected in an aircraft. In experiments carried out in the daytime the ice-nucleating properties of the silver iodide decayed by a factor of 100 in 16 min with one method of generation of the smoke, and in 100 min with another. There was no detectable decay at night. Also, the rate of rise of the smoke was so slow that it appeared likely that, if it ever entered supercooled clouds, the position at which it did so might be remote from the smoke generator and the ice-nucleating properties would be substantially reduced. The suggestion was therefore made that it might be preferable to release silver iodide from aircraft, when it would be easier to ensure that the smoke entered suitable clouds before its properties were destroyed, and that it did so in the desired place. However, it remained to be shown that silver iodide released from aircraft behaved in the same way as it did when released from the ground. Tests described in the present paper were therefore made, in which silver-iodide smoke was released from an aircraft, together with a tracer aerosol of zinc sulphide, and its properties were measured at intervals by equipment mounted in another aircraft.

2. EQUIPMENT

A silver-iodide burner was mounted under the wing of a DC-3 aircraft. It was designed by Warren and Nesbitt (1955) and is of the type used in airborne cloud-seeding operations in Australia (Adderley and Twomey, in press). Its construction is illustrated in Fig. 1. An acetone solution of silver iodide is sprayed through a swirl-type atomizer into the combustion chamber, where it is ignited by a spark plug. No separate fuel is used. Excess air enters the combustion

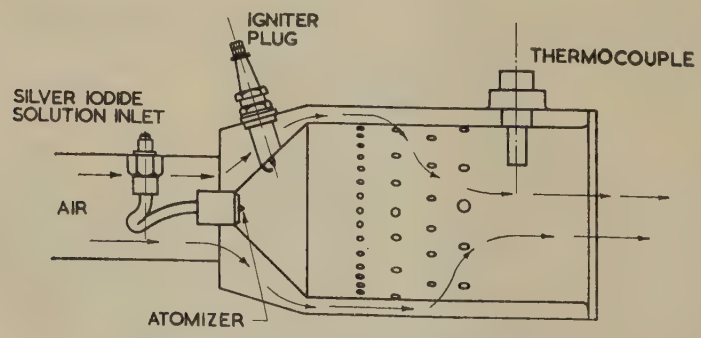

Figure 1. Silver-iodide burner – construction.

chamber through holes distributibuted over its surface and the flame is thus kept away from the chamber walls. The consumption of solution is 9 l/hr and that of silver-iodide is 550 g/hr or 2·3 g per km of aircraft flight. The solution is prepared by dissolving one part by weight of sodium iodide in five parts of acetone, then dissolving four parts of silver iodide in this solution and finally diluting with fifty parts of acetone.

Zinc-sulphide powder was dispensed from a tank under the wing of the DC-3 aircraft to which the silver-iodide burner was fitted. The powder used was fluorescent No. 2210 of the New Jersey Zinc Corporation, with approximately $5{\cdot}3 \times 10^9$ particles/g. It was carried in a hopper and dispensed by a ball mill similar to that used in the dry duster described by Braham, Seely and Crozier (1952). The powder was ejected into the air stream outside the aircraft at the rate of 220 g per km of aircraft flight, about 1·5 g being released ten times per second. The silver iodide was detected by a cold chamber and the zinc sulphide by an impactor, as previously described (Smith, Heffernan and Seely 1955).

3. Trials

The dispensing aircraft laid a trail of both types of smoke, usually 50 km long. The detecting aircraft flew backwards and forwards through this trail at right angles to it, and during each pass through the smoke a simultaneous measurement was made of the concentrations of zinc sulphide and freezing nuclei, at a point where the operator of the impactor reported that the concentration of zinc sulphide appeared to be at a maximum. An occasional air sample was taken at a position remote from the smoke trail, to check that the measurements were not confused by the presence of natural freezing nuclei.

Five flights were made over the sea and one over land, all near Sydney, Australia. The smoke was released at low altitudes, in turbulent layers below temperature inversions. There was up to 4/8 cloud cover. A total of 34 measurements was made of the concentrations of zinc sulphide and silver iodide, 22 by day and 12 by night.

4. Results of trials

The results of the measurements of the concentrations of freezing nuclei and of zinc-sulphide particles are given in Tables 1 and 2. The decay in the ice-nucleating properties of the silver iodide is displayed in Fig. 2, the daytime measurements being shown as crosses and those made at

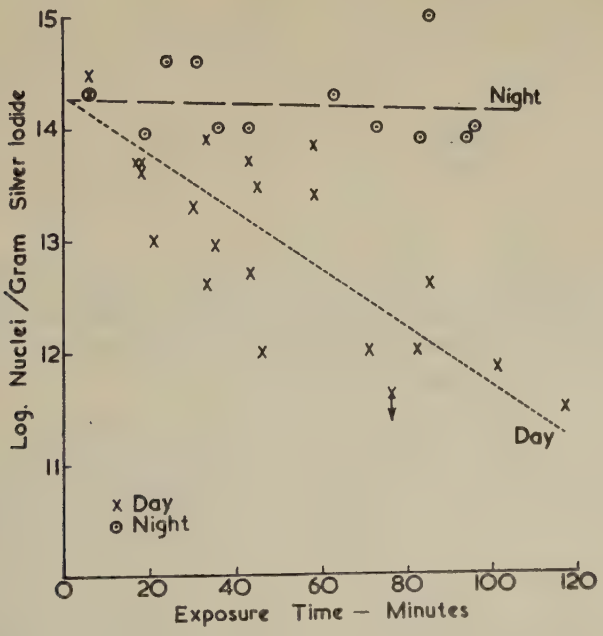

Figure 2. Freezing nuclei per gram AgI burnt vs. exposure time.

night as circles. The logarithm of the number of freezing nuclei, effective at − 17°C, per gramr of silver iodide burnt, is plotted against the time which elapsed between the release of the smoke and the measurement of concentration. The number of nuclei is computed by multiplying the output of zinc-sulphide particles by the measured ratio of the concentration of freezing nuclei to that of zinc-sulphide particles.

TABLE 1. RESULTS OF MEASUREMENTS – DAY

28 May 1956							
Time after release (min)	17	33	43				
AgI nuclei/l	20	100	3				
ZnS particles/l	0·2	0·6	0·3				
21 June 1956							
Time after release (min)	21	33	45	58	76	101	117
AgI nuclei/l	10	5	10	3	< 0·3	0·3	0·3
ZnS particles/l	0·5	0·7	0·2	0·1	0·4	0·2	0·6
26 June 1956							
Time after release (min)	6	18	30	43	58	85	
AgI nuclei/l	3000	300	50	500	30	3	
ZnS particles/l	4·7	2·9	1·4	4·4	0·2	0·4	
5 July 1956							
Time after release (min)	6	18	35	46	71	82	
AgI nuclei/l	200	30	30	10	3	3	
ZnS particles/l	0·5	0·4	1·7	3·5	1·1	1·4	

TABLE 2. RESULTS OF MEASUREMENTS – NIGHT

3 July 1956							
Time after release (min)	24	36	73	85	96		
AgI nuclei/l	1000	300	100	1000	100		
ZnS particles/l	1·2	1·4	0·4	0·5	0·4		
4 July 1956							
Time after release (min)	6	19	31	43	63	83	94
AgI nuclei/l	3000	300	1000	1000	1000	30	100
ZnS particles/l	7·1	1·6	1·4	3·6	2·6	0·2	0·6

It can be seen that the number of freezing nuclei, per gram of silver iodide burnt, which were effective after a few minutes exposure in the air, was between 10^{14} and 10^{15} both by day and by night. By day the nuclei decayed by a factor of about 1000 in 2 hr whereas by night the decay, if any, was too low to detect. These results do not differ significantly from those reported previously (Smith and Heffernan 1956) for silver-iodide smoke produced by a kerosene burner and released from the ground.

The results as displayed in Fig. 2 exhibit considerable scatter. This is due to several causes, of which probably two of the more important are errors in the estimation of ice-crystal spacing, and statistical fluctuations in small samples of both aerosols. The scatter in the daytime observations is greater than in those made by night. While this suggests that the rate of decay may depend on other factors, attempts to correlate the rate of decay with meteorological parameters such as cloud cover have been unsuccessful. The diffusion of the smoke was not investigated in any great detail, but two general observations were made. The vertical extent of the smoke trail usually grew to a value sufficient to fill the space between the ground or sea and the temperature inversion in about a half-hour, and the width of the trail increased to 2 km in ½ hr and 4 km in 1½ hr. The inversion height varied from 600 m to 2,000 m and the wind speed from 5 to 10 m/sec.

5. Conclusions

These experiments showed that the aircraft-mounted generator of silver-iodide smoke produced freezing nuclei at the rate of about 2×10^{14} per gram of silver iodide burnt, and that the nuclei decayed by day but not by night. Thus this type of burner could be used for cloud seeding by day provided that the smoke was released in such circumstances that it entered suitable clouds without too much delay: it would be suitable for target areas of limited size. If this type of burner were used at night, effects might be produced over a larger area.

Acknowledgment

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An essay on low-latitude wind analysis

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Summary

A method of wind analysis widely used in low latitudes is shown to be basically unsound, using the results of another method of analysis. It is shown that analogy may usefully be drawn between middle-latitude and equatorial airflow. A suggestion for the use of pressure data near the equator and brief comments on well-known types of wind analysis are made. The emphasis is on equatorial conditions.

1. Introduction

In this paper the word divergence means both divergence and convergence in the horizontal surface. The discussion refers, except where specified otherwise, to the tropical south-east Asian area, the main emphasis being on the equatorial zone. The purpose of this paper is to point out the basic unsoundness of the method of wind analysis described by Forsdyke (1949), Grimes (1951), Watts (1949, 1955a and 1955b) and others, and to suggest alternative procedures.

2. Description of method

There is no well-defined sequence of operations applied to the raw data. However, discussion with a number of its protagonists and impressions gathered from the literature indicate that a legitimate general description of the method – one acceptable to the protagonists – is that it essentially ' looks for divergence first.' Usually the analyst first marks lines of convergence on his chart where inspection of the observations and a variety of pre-conceived notions suggest they might exist. Some adjustments are then usually made in the pursuit of continuity both in time and the vertical, and finally arbitrarily spaced streamlines are drawn to fit observed wind directions and to indicate flow into the already assessed lines of convergence.

There are variations from this sequence. Some analysts insert streamlines first where there is good observation coverage, and continue them in roughly the same direction over blank areas until they appear to ' clash ' with lines similarly treated from other areas. This procedure and yet other variations, are still, however, those of looking for divergence first. The analyst is tacitly accepting with Grimes that ' it is possible with these charts to identify areas of convergence and divergence with reasonable reliability '; an opinion which differs markedly from that of Hare (1946) who wrote ' divergence is little more than a shibboleth used to explain the weather away.'

The writer's experience has suggested that Hare's remark is much nearer the truth than that of Grimes. The method is a most unreliable analysis tool and is of no value at all to forecasting, in the sense of foretelling changes in weather distribution.

3. Wind ' components '

Each wind observation may be regarded with complete generality as the vector sum of three vectors or ' components.'

Associated with systems of divergence of scale able to be studied by the available network there exists a field of flow without relative vorticity which is representable by the isopleths of some scalar (ξ), the local flow vector being normal to the isopleths and of magnitude inversely as their separation. This is the irrotational component.

Associated with systems of relative vorticity similarly able to be studied there exists a field of flow without divergence which is representable by the isopleths of another scalar (ψ). The local flow vector is here directed along the isopleths and is also of magnitude inversely as their separation. This is the solenoidal component.

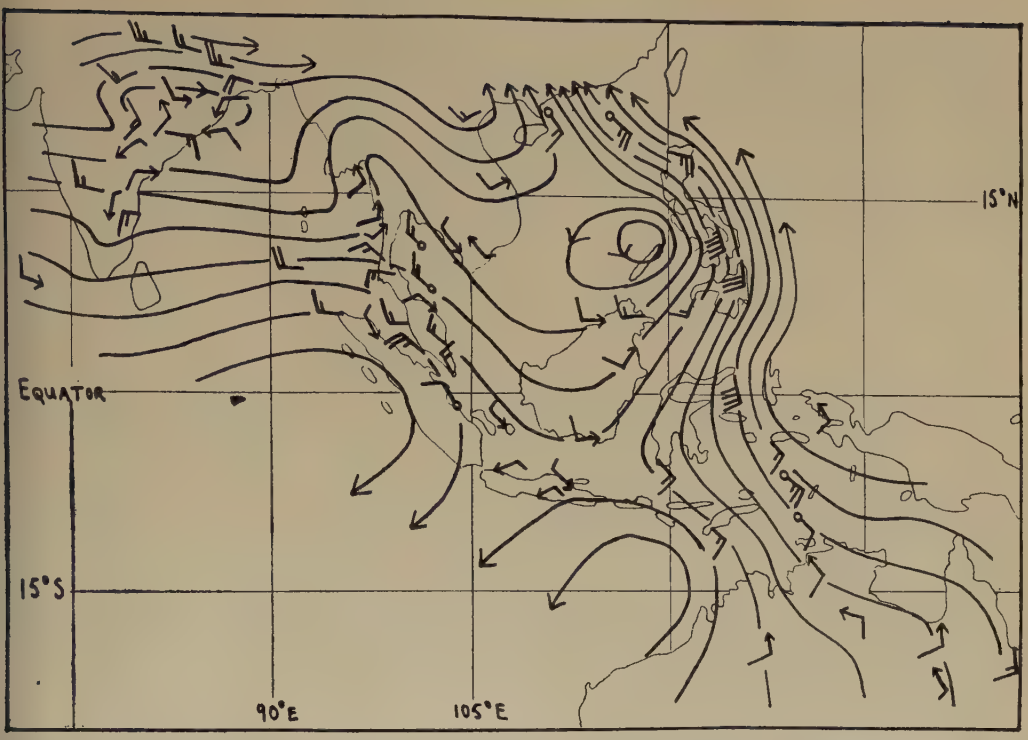

Figure 1. Solenoidal analysis of winds at 7,000 ft, 00 to 03 GMT, 12 Nov. 1949. Full flèche indicates 10 kt, half flèche 5 kt. Aircraft observations indicated by circles on ends of wind arrows. Separation of 6° indicates speed of 5 kt. Circulation to west of Philippines was classified as a tropical depression.

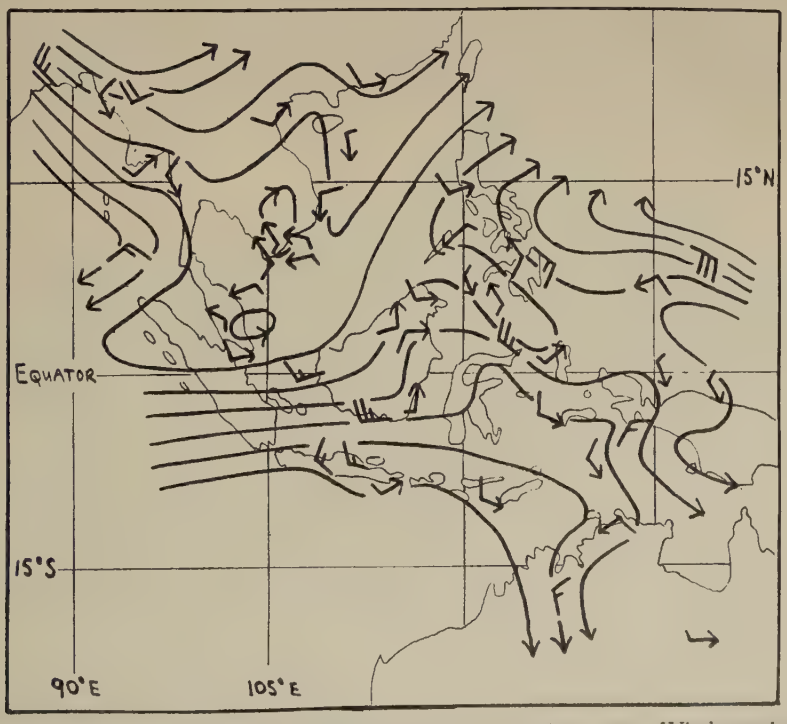

Figure 2. Solenoidal analysis of winds at 7,000 ft, 00 to 03 GMT, 13 Dec. 1949. Wind speeds indicated as under Fig. 1.

To the above components must be added an ' error ' component which is the local flow vector of the vector sum of all fields of flow contributing to disturbances of scale not able to be studied by the available network. Such fields include those associated with local showers, ordinary turbulence, differential surface friction, land and sea breezes and distortion of flow due to elevated terrain. In addition, errors of balloon computation, plotting and message transmission may be included here. It will be seen from what follows that this list is not exhaustive. The fluid elements in none of the above fields of flow are necessarily free from deformation or translation.

Expressed symbolically

$$\mathbf{V} = \nabla\xi + \mathbf{k} \times \nabla\psi + \mathbf{E} \quad . \quad . \quad . \quad . \quad (1)$$

in which $\mathbf{V}$ is the observed wind vector, ∇ gradient in the horizontal surface, $\mathbf{k}$ unit vertical vector and $\mathbf{E}$ is error component.

4. Some analytical results

Using only charts on which few or no routine observations are missing, at 5,000, 7,000 and 10,000 ft it has been found

(a) always possible to fit meteorologically simple solenoidal flow patterns to the observations (Scott 1953) with only minor smoothing, and

(b) never possible to analyse such a set similarly after rotating all its observations through right angles in the same sense.

Meteorologically simple means a pattern without a complex of whirls and vortices of small extent and giving no reason to suppose that it does not provide adequate interpolation between the observations. Minor smoothing means that vector differences between observations and the winds indicated by the pattern are unsystematically distributed and generally small. Large vector differences, if required by the technique, were allowed either when there was independent reason to suspect an observation (local distortion of flow, etc.), or in pursuit of what might be called ' natural smoothing.' For example, where a group of relatively close observations differs little in direction it is natural, in the absence of reasons to do otherwise, to smooth the pattern associated with them into a nearly uniformly moving airstream. This type of smoothing involved at times vector differences of the order of 10 kt.

There were over 100 test occasions, including the typhoon data given by Thompson (1951) in his Figs. 5, 6, 7 and 8. Figs. 1 and 2 are examples of the successful type of analysis. The analysis over Malaya and North Sumatra in Fig. 1 is an example of natural smoothing.

Attempts to use this type of analysis at levels below 5,000 ft on both observed and rotated winds were not satisfactory. At these lower levels component and isogon analyses, in a shorter test involving equatorial weather disturbances only, agreed with one another satisfactorily but failed to locate known areas of bad weather.

5. Discussion

For a technique which seeks divergence first to be sound it is necessary and sufficient that the irrotational components of the wind observations be dominant, i.e., the irrotational component should comprise the bulk of a ' non-light ' observation. If this is true, sets of rotated winds would be analysable into simple solenoidal flow patterns and sets as observed not. This however has not been found true at any level, and it is concluded that the technique based on seeking divergence first is unsound.

On the other hand (ignoring for the moment the occasional large natural smoothing needed) it is not legitimate to conclude from the results of solenoidal analysis at 5,000 ft to 10,000 ft alone that solenoidal components are dominant there. Evidence is also needed that irrotational components can only reach magnitudes of the order of a few knots. The figures given by Scott (1953) support this. Support is also given by the frequency with which patterns predict late observations totally unexpected by ordinary methods of extrapolation, and the generally convincing nature of the patterns produced. With regard to typhoons there is little doubt that solenoidal components are dominant above the shallow layer of strong convergence near the surface and below the divergent layers in the upper troposphere.

It appears then reasonable to accept that it is a general rule that at these levels solenoidal components are dominant. It follows that the relation $\mathbf{V} = \mathbf{k} \times \nabla\psi$ has a validity similar to that of the geostrophic relation of middle latitudes, and the field of ξ is much flatter than that of ψ. In general therefore relative vorticity ($\nabla^2 \psi$) reaches a higher order of magnitude than divergence ($\nabla^2 \xi$).

Similar conclusions are probably valid at levels above 10,000 ft not too near the tropopause, but it has not been possible to test because of the relatively few observations generally available at these levels. At levels below 5,000 ft no similar simplification appears possible. Also, the failure of the quantitative types of analysis suggests that error components here become large enough to prevent any type of wind analysis being consistently useful for detecting weather directly in the equatorial regions.

On the other hand, solenoidal analysis at levels above the confused lower layers does appear basically sound. In addition to the tests described above it has been used at 7,000 ft as a routine at the Forecast Office, Singapore for some months. Experience of its use may be summarized as follows :

(i) Its value for quantitative interpolation between observations is accepted.

(ii) It succeeds in locating occasional extensive bad-weather areas not too far from the equator by their association with relative vorticity larger than any in the vicinity on previous charts. This is a coarse application of the vorticity equation. The illustration in Scott (1953) is the final chart of such a sequence.

(iii) Fine applications of the vorticity equation (e.g. assessment of weather distribution in a moving-wave type of disturbance) are not generally possible. This failure is attributable in part to the coarse network of observations available, but it is felt that improvement could be effected with existing facilities by using winds meaned over a much thicker layer than the 500 ft thick layer used at present. It has been found over Malaya on a number of suitable occasions that the natural smoothing required was considerably reduced when the normal observations were replaced by winds computed through a 2,500 ft thick layer. This type of error can be convincingly smoothed out over a group of observations, but has to remain in isolated ones where it can substantially alter an analysis and therefore spoil a sequence.

There is also prospect of relating the patterns with the surface-pressure field; a development advocated by Grimes. For well-known reasons single sets of synoptic pressure observations have proved useless for this purpose. However, working on the lines indicated in Scott (1955) the writer has now obtained statistically significant results from time-smoothed pressure data, which promise value in charts of daily or half-daily mean pressure observations. In these latter together with solenoidal wind analysis appear the best prospects for forecasting in equatorial regions using data not at present out of reach. The findings of Sharp (1957) are not relevant here, there being no relation in equatorial regions between tendencies of pressure and those of its Laplacian.

6. An African example

Forsdyke compares an analysis of the type under criticism with a quantitative component analysis of the distribution of divergence at 2 km over East Africa and adjacent territories. The results of the quantitative analysis on this occasion with the network available are disappointing. Of the five areas assessable by weather observations between 30°N and 30°S where Forsdyke found significant divergence, in only one was it unnecessary to call upon other reasons to explain the weather. The writer finds it particularly difficult not to suspect the analysis where it indicates marked convergence associated with the rapid spreading of an anticyclone; an occurrence not unlike the result of a computation made by Richardson (1922). Since also it is not difficult to extract from a middle-latitude chart a set of *geostrophic* winds which appear by component analysis to have lost significantly the property of solenoidal flow, such analysis may reasonably be suspected of exaggeration and misrepresentation.

A similar remark is even more obviously applicable to streamline and isotach analysis. Here the comparative objectivity of the isogon field is removed by conversion to streamlines by a subjective process whose limitations often appear to be inadequately understood. There appears no other explanation for the many fantastic analyses appearing in recent literature (e.g. Bath, Lloyd and Ryan 1956), and an assertion (Palmer 1951) that, in addition to low-level convergence, a cloud line must always coincide with a streamline – a condition ensuring its transverse immobility.

On the other hand, the observations used by Forsdyke responded reasonably well to the solenoidal pattern technique, though there must be doubt, owing to the nature of the terrain, that the properties claimed for 5,000 ft. and above in south-east Asia apply here. The analysis is given in Fig. 3. The technique again failed with the winds rotated.

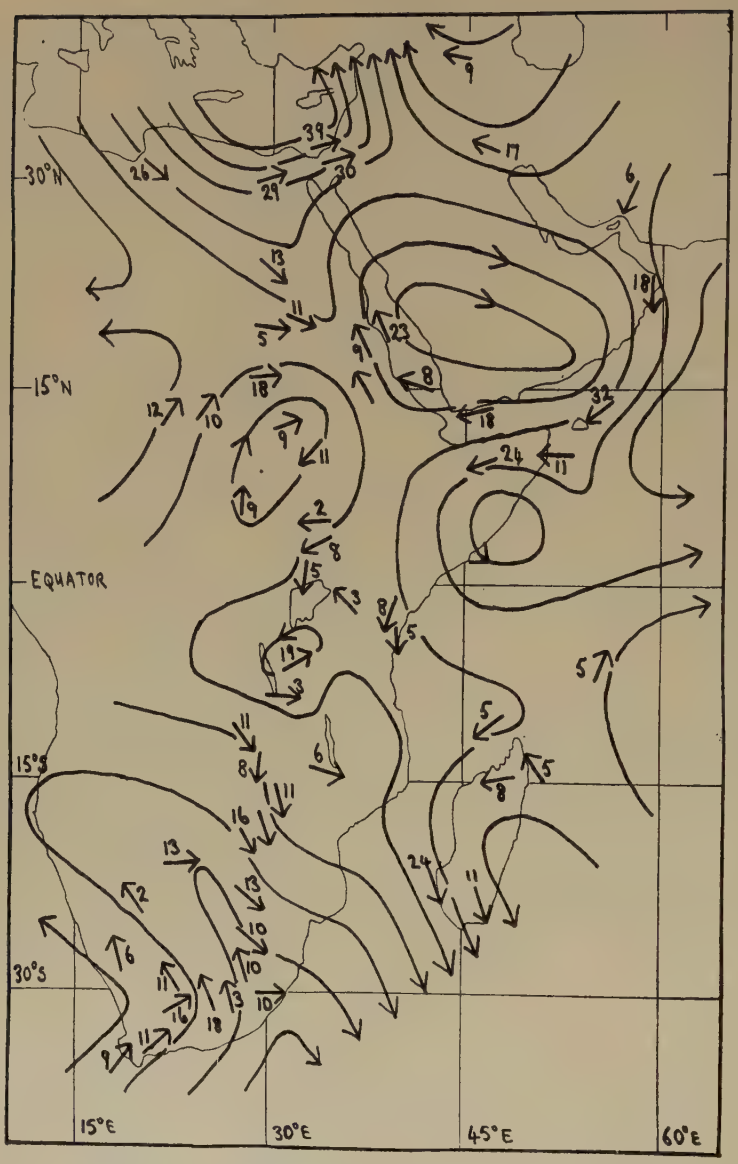

Figure 3. Solenoidal analysis of winds at 2 km on the morning of 24 Nov. 1944. Wind speeds in knots. Separation of 9° indicates speed of 5 kt.

7. Conclusion

A prerequisite for the soundness of a method of wind analysis which looks for divergence first is that the irrotational components of wind observations at all levels analysed should be dominant. No evidence for this has been found at any level in tropical south-east Asia. On the contrary it appears probable that, from 5,000 ft to 10,000 ft at least, winds are extractable whose solenoidal components rarely differ by more than a few knots from the observations themselves. This coupled with the probably large values attainable by error components below 5,000 ft makes it appear unlikely that any method of analysis will permit the detection in the equatorial regions of horizontal divergence directly, consistently and accurately (even though it is never difficult to fit the winds into a qualitative pattern indicating convergence into reported bad-weather areas).

Indirect assessment of divergence through the vorticity equation does, however, appear to have possibilities, and the forecasting potentialities of the surface pressure field appear worth further investigation.

Acknowledgment

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The empirical relation between solar radiation and hours of sunshine

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SUMMARY

The variation with latitude of the regression parameters a and b in the conventional equation of Ångström type

$$Q/Q_A = a + bn/N$$

between total solar radiation on a horizontal surface (Q) and hours of bright sunshine (n) has been examined. It has been shown that the slope of the relationship b is effectively constant, while the threshold value a is a function of latitude. An empirical relationship leading to the Ångström equation at each latitude ϕ over the range $0° - 60°$ is derived and is shown to give values in reasonable accord with previous experience. It is

$$Q/Q_A = 0{\cdot}29 \cos \phi + 0{\cdot}52\, n/N$$

1. INTRODUCTION

In a previous paper (Glover and McCulloch 1958) it was shown that the sum of the parameters in the non-dimensional equation of the Ångström type,

$$Q/Q_A = a + bn/N \quad . \quad . \quad . \quad . \quad (1)$$

between total solar radiation on a horizontal surface Q and hours of bright sunshine n, was closely related to latitude through optical air mass m, represented by the mean noon value at the equinoxes. In Eq. (1), Q_A and N have their usual meanings; Q_A is the radiation per unit area of horizontal surface at the upper limit of the atmosphere and N is the total possible hours of sunshine. Using the data from six stations ranging in latitude from 0 to 52°, the equation

$$t^m = 0{\cdot}99 - 0{\cdot}17\, m, \quad r^2 = 0{\cdot}90 \quad . \quad . \quad . \quad . \quad (2)$$

where t^m, that is $(a + b)$, is the transmission coefficient at optical air mass m, enabled the data from these widely scattered stations to be compared. It was clear however that further data would be essential before a reliable relationship would exist for calculation of the actual regression constants.

2. FURTHER DATA

A series of five radiation stations was set up from 1951 onwards in the Union of South Africa (Drummond and Kirsten 1951). Additional data were also obtained from Kew, England. This material was analysed by grouping in daily, ten-day and monthly periods and the resulting values of the regression constants were found to vary according to the type of grouping, as was shown for the Kabete data (Glover and McCulloch 1958). Whilst it might appear that no one method of grouping would be superior to another, and that the differences resulting are entirely statistical (and indeed, usually non-significant statistically), in fact it was found that taking a mean of 10 consecutive days or one calendar month sometimes caused a considerable restriction in the range of values used in the regressions. As an example of the restriction of the independent variable, individual daily values of n/N at Kew, over the period 1947-1954, ranged from zero to unity; when arranged by months the maximum value was only 0·58. It was therefore decided to use, where possible, only regressions calculated from daily values, so that the resulting parameters from all over the world would be more strictly comparable.

3. VARIATION OF $a + b$

The regression constants for each year, calculated from daily records, are given in Table 1. Data for each year have been used, since the period of operation of the stations differed, and it is clear that considerable variation in the values of the coefficients at any one station can occur from year to year. Table 1 includes the mean annual optical air mass and latitude of each station.

TABLE 1. THE RELEVANT POSITIONAL AND REGRESSION DATA FOR THE STATIONS EXAMINED

Station	a	b	$a+b$	m	Lat. $\phi°$	$\cos\phi$
Kabete	0·24	0·55	0·79	0·81	1·3 S	1·00
	0·27	0·55	0·82			
	0·29	0·54	0·83			
	0·26	0·58	0·84			
	0·24	0·59	0·83			
Windhoek	0·25	0·55	0·80	0·90	22·6 S	0·92
	0·27	0·52	0·79			
	0·28	0·49	0·77			
	0·25	0·52	0·77			
Pretoria	0·24	0·52	0·76	0·95	25·8 S	0·90
	0·26	0·50	0·76			
	0·27	0·49	0·76			
	0·25	0·49	0·74			
	0·22	0·52	0·74			
Bloemfontein	0·27	0·48	0·75	0·96	29·1 S	0·87
	0·25	0·51	0·76			
Durban	0·27	0·54	0·81	1·15	29·8 S	0·87
	0·26	0·51	0·77			
	0·27	0·49	0·74			
	0·26	0·49	0·75			
	0·24	0·51	0·75			
Wingfield	0·26	0·52	0·78	1·20	33·9 S	0·83
	0·30	0·51	0·81			
	0·30	0·48	0·78			
	0·28	0·51	0·79			
Kew	0·20	0·60	0·80	1·62	51·5 N	0·62
	0·20	0·51	0·71			
	0·19	0·50	0·69			
	0·16	0·53	0·69			
	0·15	0·51	0·66			
	0·16	0·49	0·65			
	0·15	0·53	0·68			
	0·17	0·50	0·67			
	0·15	0·54	0·69			

A linear relation of the type of Eq. (2) between the sum of $a + b$ and the optical air mass was first sought, using the new data in Table 1 only. The equation was

$$t^m = 0{\cdot}90 - 0{\cdot}13\, m, \text{ for 34 values, } r^2 = 0{\cdot}58 \quad . \quad . \quad . \quad (3)$$

This relation does not differ significantly from the previous Eq. (2) though, owing to the larger number of observations and to the use of annual values derived from daily data rather than those derived from mean monthly data averaged over several years, a marked drop in the amount of accountable variance has resulted. A slight, though insignificant increase in the accountable variance was obtained by using a relationship between t^m, and $\cos\phi$ where ϕ is the latitude of the station

$$t^m = 0{\cdot}51 + 0{\cdot}30 \cos\phi, \text{ for 34 values, } r^2 = 0{\cdot}64 \, . \quad . \quad . \quad (4)$$

4. DEPENDENCE OF b ON POSITION

Until it is possible to predict the variation with latitude or air mass of a and of b, in addition to that of $a + b$, Eqs. (3) and (4) must remain of value only for comparing the results calculated for different stations. Therefore the variation of the individual parameters was studied. From Table 1 it is clear that b remains comparatively constant, and no significant dependence of b on either optical air mass or latitude was found by fitting a linear regression to the data. In fact the

variance accountable in this way was only 2 per cent. The distribution of values of b was therefore examined using Fisher's method of moments (1948) but no significant departure from normality could be detected from the 34 records available. It was concluded that for all practical purposes b could be considered constant, with a mean value of 0·52 and a standard error of 0·005. This is in good agreement with the observation of Black, Bonython and Prescott (1954) that b is approximately constant between 0·50 and 0·55, with no evidence of dependence on latitude.

5. Dependence of a on position

Values of a in Table 1 appear to depend quite markedly on latitude, so regressions on optical air mass, and on latitude and altitude were obtained. Both of these regressions were very significant ($P = 0{\cdot}001$) though the multiple regression on latitude and altitude gave a better fit. Of a total accountable variance of 75 per cent, in this latter regression, some 66 per cent was provided by latitude and the remainder by the altitude effect. However no significant dependence of $a + b$ on altitude could be discovered, owing to the random variation of b and to the paucity of records. Therefore the altitude effect has been neglected, and there remains the relation

$$a = 0{\cdot}01 + 0{\cdot}27 \cos \phi \quad . \quad . \quad . \quad . \quad (5)$$

This is very similar to the result obtained by substituting the average numerical value of b in Eq. (4) which gives

$$a = -0{\cdot}01 + 0{\cdot}30 \cos \phi \quad . \quad . \quad . \quad . \quad (6)$$

and to simplify the resulting Ångström equation, the relation adopted was

$$a = 0{\cdot}29 \cos \phi \quad . \quad . \quad . \quad . \quad . \quad (7)$$

This provides an adequate empirical relationship over the latitude range $0 - 60°$. The equation of Ångström type, over this limited range of latitude then becomes

$$\frac{Q}{Q_A} = 0{\cdot}29 \cos \phi + 0{\cdot}52\, n/N \quad . \quad . \quad . \quad (8)$$

6. Application to additional data

Eq. (8) has been derived from the annual regression constants, a and b, calculated from daily values of radiation and hours of sunshine. To compare the values calculated from the equation with those obtained in practice, the value of the regression constants for each station derived from daily records for the whole period for which records are available has been used in Table 2. However, for five of the stations mentioned in our previous paper (1958), values of a and b derived from monthly means have also been quoted, on the assumption that, in the absence of data which are strictly comparable, the monthly data will at least provide a reasonable estimate of the data from daily values. Each of these stations is denoted by an asterisk.

TABLE 2. Comparison of calculated and observed constants

Station	Lat.°	Calculated			Observed		
		a	b	t^m	a	b	t^m
Kabete	1·3 S	0·29	0·52	0·81	0·26	0·57	0·83
Windhoek	22·6 S	0·27	0·52	0·79	0·26	0·52	0·78
Pretoria	25·8 S	0·26	0·52	0·78	0·25	0·50	0·75
Bloemfontein	29·1 S	0·25	0·52	0·77	0·25	0·50	0·75
Durban	29·8 S	0·25	0·52	0·77	0·26	0·50	0·76
Wingfield	33·9 S	0·24	0·52	0·76	0·29	0·50	0·79
Kew	51·5 N	0·18	0·52	0·70	0·17	0·52	0·69
Gembloux*	50·6 N	0·18	0·52	0·70	0·15	0·54	0·69
Versailles*	48·8 N	0·19	0·52	0·71	0·23	0·50	0·73
Mount Stromlo*	35·3 S	0·24	0·52	0·76	0·25	0·54	0·79
Dry Creek*	34·8 S	0·24	0·52	0·76	0·30	0·50	0·80
Rothamsted*	51·8 N	0·18	0·52	0·70	0·18	0·55	0·73

The relation (8) can also be applied to derive the Ångström equation for Stockholm, to compare it with that postulated by Ångström (1928). His equation was written in terms of Q_0 where Q_0 is the maximum possible radiation on a horizontal surface on a clear cloudless day. If, following Penman (1948), we assume that $Q_0 = t^m Q_A$, it is easy to compare the equations. By (8) for latitude 59·4°N

$$\frac{Q}{Q_A} = 0{\cdot}15 + 0{\cdot}52\, n/N \quad . \quad . \quad . \quad . \quad (9)$$

In the original Ångström form Eq. (9) may be written

$$\frac{Q}{Q_0} = 0{\cdot}22_5 + 0{\cdot}77_5\, n/N$$

which compares favourably with the equation given by Ångström

$$\frac{Q}{Q_0} = 0{\cdot}23_5 + 0{\cdot}76_5\, n/N$$

Black *et al* (1954) provide a list of records from 32 stations ranging in latitude from 6° to 65°. They derived, from the massed data, an equation

$$\frac{Q}{Q_A} = 0{\cdot}23 + 0{\cdot}48\, n/N$$

which is the best relation obtainable from these admittedly rather unsatisfactory data. Taking as the mean latitude 37°N, which lies in the central group containing the greatest number of stations, Eq. (8) would provide an estimate

$$\frac{Q}{Q_A} = 0{\cdot}23 + 0{\cdot}52\, n/N$$

7. Conclusion

The data examined above are in reasonable agreement with the empirical relation (8). Whilst in Table 2 numerical differences of up to 0·04 occur between calculated and observed values of $(a + b)$ and the standard deviation of the differences is 0·03, these slight differences are of little consequence in practice, for they show up only at the high and low extremes of radiation or hours of sunshine. It must be remembered that different groupings of the same data for one station can lead to differences in regression constants of the same order. The physical background of Eq. (8) is being investigated but as yet no completely satisfactory explanation has been found. Whilst little confidence can be placed on the second decimal place of the constants of the Ångström-type equation, whether calculated from the full data for a station over a period of years or estimated from Eq. (8) for areas where data are unreliable or non-existent, the empirical relation should be a valuable tool for the agricultural meteorologist.

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551.506.2 (99) : 551.506.5 (99

Summary of observations at Shackleton, Antarctica 1956-57

By R. H. A. STEWART
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(Manuscript received 16 October 1957)

1. Introduction

Shackleton, the Weddell Sea base of the Trans-Antarctic Expedition, was established in position 77° 59′ S, 37° 10′ W on 29 Jan. 1956. Its position is about 1,000 yd inland from the front of the Filchner Shelf Ice and at an altitude of 190 ft (58 m). To the west and south the shelf is continuous and unbroken by high ground for at least 200 mi. To the east the self is bounded by the hills (ice-covered except for a few minor rock exposures) running south from Vahsel Bay (77° 50′ S, 34° 40′ W). At their closest these are about 20 mi from Shackleton and their general heights about 2,000 ft (600 m).

This area was first visited by Filchner in 1912. He was unsuccessful in establishing a shore base but for a short period observations were made by the ship and by shore parties (Filchner 1922). Shackleton approached the area in 1915 but the *Endurance* was beset before reaching the coast (Shackleton 1919). In January 1955, an Argentine base was established in position 77° 50′ S, 39°W and has been manned since that date. The meteorological data have not been published.

2. Notes on instruments and observations

(*a*) *General*

Observations began at Shackleton on 1 Mar. 1956 and continued until November 1957. Tables 1 to 6 contain a summary of certain observations during 1956, together with some information received by radio for January and February 1957. The monthly means are based on observations at synoptic hours. The number of these increased from five daily in March and April to the full eight from 1 Sept.

(*b*) *Pressure*

Pressure was measured by a Fuess precision aneroid barometer which was checked regularly by a hypsometer. Prior to September, while the party was living in a temporary hut, the instrument was subject to large and rapid temperature fluctuations with resulting condensation. The observations have been reduced to sea level using a barometer height of 190 ft (58 m). This height is subject to revision when final levelling is completed.

TABLE 1. Pressure at mean sea level (mb)

1956	Monthly mean	Extremes			
		Max.	Date	Min.	Date
Mar.	–	–	–	–	–
Apr.	997·4	1019·8	29	983·2	19
May	1004·3	1030·6	2	984·5	22
June	994·4	1022·1	15	964·5	6
July	995·5	1029·1	23	965·3	2,6
Aug.	995·4	1013·2	1	970·7	9
Sept.	993·3	1013·7	20	958·3	3
Oct.	986·7	1001·6	10	972·2	19
Nov.	988·9	1006·5	21	961·6	9
Dec.	998·3	1009·7	20	978·0	1
1957					
Jan.	998	–	–	–	–
Feb.	993	–	–	–	–

(c) *Temperature*

The thermometers were exposed in a standard Stevenson screen. This was moved a short distance on two occasions during the year to avoid drifts spreading from surrounding objects. No modification of the screen was attempted to decrease the effect of reflected radiation from the snow surface as such modification seemed likely to increase the already considerable difficulty experienced in keeping the interior of the screen clear of snow. Monthly mean temperatures before September are the mean of the 00 GMT, 12 GMT, daily maximum and daily minimum temperatures. The maximum and minimum temperatures are based on values for each 24-hour period ending at 2200 hr zone time.

TABLE 2. TEMPERATURES (°C)

1956	Monthly mean	Max. Temperature mean	Max. Temperature extreme	Min. Temperature mean	Min. Temperature extreme
Mar.	− 21·1	− 17·3	− 6·7	− 25·8	− 41·7
Apr.	− 26·6	− 21·9	− 4·5	− 31·2	− 44·5
May	− 37·3	− 31·1	− 16·8	− 42·9	− 51·4
June	− 27·0	− 21·3	− 8·1	− 32·7	− 46·2
July	− 31·3	− 25·0	− 10·8	− 37·5	− 50·6
Aug.	− 33·2	− 28·3	− 10·5	− 25·0	− 53·0
Sept.	− 27·5	− 23·5	− 6·2	− 32·3	− 43·2
Oct.	− 23·0	− 19·1	− 7·3	− 27·7	− 39·4
Nov.	− 10·2	− 7·7	− 1·4	− 13·5	− 20·8
Dec.	− 4·4	− 1·7	+ 2·6	− 7·5	− 12·2
1957					
Jan.	− 8·0	− 5·6	− 0·3	− 10·9	− 20·2
Feb.	− 11·8	− 9·0	− 1·4	− 15·2	− 29·1
Year	− 21·8	− 17·6	–	− 25·2	–

(d) *Wind speed and direction*

Until 1 Nov. hand anemometers were in use and wind directions were estimated. From 1 Nov. a cup generator anemometer and a remote indicating wind vane were in use on a 31-ft lattice tower, recording on an electrical anemograph in the meteorological office. The speeds for the earlier months have been corrected to 33 ft using correction figures obtained by direct comparison of instruments at 6 ft and 33 ft during the first half of 1957.

TABLE 3. SURFACE WIND (33 ft ≃ 10 m)

1956	Mean speed (kt)	Direction (percentage frequency) degrees (true) 320/040	050/130	140/220	230/310	Calm
Mar.	17·5	24·0	3·3	72·0	0·7	0
Apr.	15·5	18·2	1·4	78·4	0	2·0
May	22·2	12·0	2·0	86·0	0	0
June	17·0	36·7	12·0	47·3	0	4·0
July	18·1	25·9	5·9	66·2	2·0	0
Aug.	22·0	33·4	3·2	62·9	0·5	0
Sept.	22·3	30·0	2·9	66·7	0	0·4
Oct.	15·6	26·6	8·4	63·4	0·4	1·2
Nov.	15·6	53·3	17·1	28·8	0	0·8
Dec.	9·9	29·1	26·2	38·3	2·8	3·6
1957						
Jan.	10·7					
Feb.	9·7					
Year	16·3					

TABLE 4. SURFACE WIND : SPEED > 33 KT

1956	No. of obs.	Percentage of all obs.	Mean speed (kt)	Direction frequency (per cent) degrees tru 320/040	050/130	140/220	230/31
Mar.	15	9·4	36·8	0	0	100·0	0
Apr.	1	0·7	35·0	0	0	100·0	0
May	15	10·0	35·8	0	0	100·0	0
June	18	12·0	42·9	66·7	0	33·3	0
July	19	12·7	38·6	36·8	0	63·2	0
Aug.	46	24·7	42·7	65·2	0	34·8	0
Sept.	50	20·8	41·7	46·0	0	54·0	0
Oct.	19	7·7	40·0	84·2	0	15·8	0
Nov.	7	2·9	36·0	85·7	14·3	0	0
Dec.	1	0·4	35·0	100·0	0	0	0

(e) *Sunshine and cloud amount*

The Fuess universal sunshine recorder was mounted on the roof of a low building. Loss of recording owing to exposure effect has not yet been calculated but is thought to be very small. Small losses may also have occurred owing to icing up of the glass sphere.

TABLE 5. CLOUD (EIGHTHS) AND SUNSHINE (HR)

1956	Mean total Cloud amount	Sunshine Total	Sunshine Percentage of possible
Mar.	5·2	97·3	24·0
Apr.	5·0	18·4	15·6
May	2·8		
June	4·8		
July	3·6		
Aug.	4·3	3·1	7·4
Sept.	5·4	49·5	16·2
Oct.	5·7	181·5	30·8
Nov.	6·1	195·8	27·2
Dec.	5·7	275·1	37·0
1957			
Jan.	5·7	314·2	42·2
Feb.	5·6	283·6	48·1
Year	5·0		
Total		1418·5	

Sun below the horizon from 22 April to 22 August.

(f) *Miscellaneous observations*

(i) *Fog.* Owing to lack of satisfactory humidity observations for much of the year, no attempt has been made to distinguish ' true ' fog from reduction of visibility due to drifting snow. All occasions with horizontal visibility less than 1,100 yd (1 km) have been included in the figures in Table 6.

(ii) *Drifting snow.* Drift is described as ' high ' when it causes a deterioration in horizontal visibility at eye level.

TABLE 6. Miscellaneous observations

	Number of days				
1956	Snow	Gale	Visibility < 1 km	Drift snow	High drift snow
Mar.	14	5	17	24	18
Apr.	7	1	15	20	13
May	3	7	17	30	25
June	19	5	14	20	11
July	9	7	14	24	17
Aug.	14	11	21	29	24
Sept.	16	12	22	27	21
Oct.	13	5	14	25	17
Nov.	21	3	14	22	14
Dec.	12	1	5	15	5

Acknowledgments

The writer wishes to acknowledge the help of Mr. P. H. Jeffries of the Meteorological Office and Mr. J. la Grange of the South African Weather Bureau, his fellow meteorologists at Shackleton, and to thank the Committee of the Trans-Antarctic Expedition for permission to publish this provisional summary.

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Correspondence

551.551.5 : 551.557.2 : 656.

Anisotropic high-level turbulence

By A. E. Parker

I read the recent paper by Mr. Clodman (*Q.J.*, **83**, p. 116, 1957) with great interest but fee that his conclusions do not necessarily follow from the facts.

Mr. Clodman deduced, from the fact that the degree of bumpiness varied with direction of flight for a number of flights, that the gusts producing the bumpiness were horizontal. However if the gusts had been vertical and distributed in horizontal and parallel lines, then it is reasonable to suppose that the degree of bumpiness would vary with direction of flight.

Let us for the moment assume, with Mr. Clodman, that the gusts were horizontal, and make an estimate of their magnitude. In case 5 (Table 1) of Mr. Clodman's paper the aircraft met severe turbulence which would correspond to a vertical acceleration of at least 0·5 *g*. If the gust consisted of an instantaneous increase in horizontal wind speed of v and the true airspeed of the aircraft was V then the vertical acceleration produced would be

$$\left\{\frac{2v}{V} + \left(\frac{v}{V}\right)^2\right\} g$$

This follows because the lift varies as the square of the airspeed at constant angle of incidence, provided that the airspeed is not greater than, say, about half the speed of sound.

Now take the acceleration produced as 0·5 *g* and we have

$$\frac{2v}{V} + \left(\frac{v}{V}\right)^2 = 0{\cdot}5$$

from which

$$\frac{v}{V} = 0{\cdot}225$$

Mr. Clodman did not give the airspeeds of the aircraft engaged in his investigation so a range of possible speeds will be taken and the gust speeds necessary to produce 0·5 *g* acceleration calculated. The results are given in the following Table. The values of v were calculated directly from $\frac{v}{V} = 0{\cdot}225$. The figures in brackets were obtained when allowance was made for the fact that the lift does not vary exactly as the square of the airspeed at high speeds.

True airspeed of aircraft V (kt)	Gust speed v to produce 0·5 *g* acceleration (kt)	
200	45	
300	67·5	(58)
400	90	(65)

It will be seen from the Table that the horizontal gust speeds required to produce severe turbulence are large; in fact they seem too large to be probable. Now real gusts take a short time in which to build up so would have to be somewhat larger than the calculated values to give the same acceleration.

There is a further objection to Mr. Clodman's theory. If these large horizontal gusts do give turbulence, then variations in airspeed of, say, 40 to 90 kt ought to occur but I have never heard of such large variations taking place, although smaller variations would be expected, especially near the ground.

It is always assumed by aircraft designers and stressmen that bumpiness is caused by vertical gusts. Horizontal gusts are comparatively ineffective in producing bumpiness. For example, in the case of the Comet 1 flying at 34,000 ft and at a true airspeed of 380 kt a vertical sharp-edged gust of 9 kt would cause a vertical acceleration of 0·5 *g*, i.e., severe turbulence. A sharp-edged horizontal gust of 9 kt would give an acceleration of only 0·05 *g*, i.e., very slight turbulence.

In conclusion it seems that the gusts which cause most aircraft bumpiness are vertical gusts. Horizontal gusts, of the magnitude ever likely to be encountered, can only cause slight turbulence.

I wish to thank the Director-General of the Meteorological Office for permission to publish this letter.

Meteorological Office,
Air Ministry,
Dunstable, Beds.
3 October 1957

Reply

By J. Clodman

I was pleased to see Mr. Parker's comments on my paper since they represent the orthodox view that horizontal gusts cannot be strong enough to cause significant aircraft bumpiness. However, before proceeding to this point, Mr. Parker's alternative explanation of the phenomenon reported in my paper deserves a few words.

In order that vertical gusts distributed in horizontal and parallel lines should cause a marked directional variation in intensity of bumpiness, each line of gusts would have to move simultaneously up or down. Otherwise only a variation in bump frequency would be noted. A little thought as to a realistic atmospheric situation which would have the above properties, and be properly aligned with the wind, points to what is essentially a gravity-wave condition. I mentioned in my article that this was the only satisfactory alternative explanation but indicated that the weight of the evidence was in favour of horizontal gusts as the cause of the directional variability in turbulence intensity.

Mr. Parker has derived some figures as to the required intensity of horizontal gustiness necessary to cause the observed phenomenon. I believe that the velocities quoted by him are much too high. Mr. Parker assumes that a vertical acceleration of 0·5 *g* is required to give the reported turbulence intensity. After examining a large body of data the Thunderstorm Project (Byers *et al.* 1949) concluded that, for a complete traverse of a thunderstorm, heavy (or severe) turbulence was usually reported if the mean maximum effective gust velocity exceeded 8 ft sec^{-1} and the frequency exceeded 4 gusts per 3,000 ft of traverse. They indicate also that the gust velocity required to be reported as heavy is reduced if the gust frequency is increased and if the length of traverse in turbulent air is increased. Thus, for high-level turbulence with a large traverse in turbulent air and very great gust frequency the value of the minimum equivalent vertical gust velocity would be nearer to 6 ft sec^{-1} or about 3½ kt (see Clodman 1953). This does not take into account the likelihood that some of the turbulence may be due to vertical gusts since there is residual turbulence in the minimum direction. Using the above data we see that the horizontal gust velocities required are of the order of one-third to one-half those given by Mr. Parker. For the aircraft involved horizontal gusts of 30 kt would be ample to produce the observed results. It should be pointed out that the variability of pilot-reporting on such matters is so great that for individual cases such as given in my paper this value could easily be lower.

This intensity of gustiness does not, of course, have to be the normal situation but need only occur on occasion. I would, in fact, be surprised if such horizontal gust velocities do not occur. They are found near the surface of the earth with mean wind strengths of 40 kt (Thomas 1953). Why then should they not occur at high levels with several times the mean wind velocity? Even vertical gusts of this magnitude have occasionally been reported. I carried out a calculation based on Godson's (1950) study of the motion of disturbed parcels and, assuming large but realistic values of small-scale horizontal shears, was able to indicate that horizontal gusts of the necessary strength were indeed possible.

Mr. Parker presents one piece of evidence to indicate that strong horizontal gusts do not occur. This involves the fact that pilots do not report such fluctuations in their airspeed indicators. Pilots will seldom watch the indicator to see if such fluctuations occur. However, the major reason for the absence of such reports is the inability of the instrument to respond to such short-period variations. Even at very low levels where the period of the gusts is much larger

and the airspeed has been reduced, only small fluctuations are noted. Nonetheless, an examination of anemograph tracings will readily convince one that such gusts do occur here. At high levels it takes on the average about one-third of a second for the growth and decay of a gust (Clodman 1953). Only a small fraction of the maximum gust velocities could be recorded under these conditions.

In conclusion I would like to suggest that we should be careful not to reject a new concept largely because it goes against our intuitive feelings on the matter.

I wish to thank the Director of the Meteorological Service of Canada for permission to publish this letter.

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Meteorological Service of Canada,
315 Bloor Street West,
Toronto, Ontario.
13 December 1957.

551.558.21

Airflow over mountains: indeterminacy of solution

By R. S. Scorer

The two-dimensional flow over a ridge whose height is given by

$$\zeta_1 = \int_0^\infty F(k) \cos kx \, dk \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$= \text{real part of} \int_0^\infty F(k) \, e^{ikx} \, dk \quad . \quad . \quad . \quad . \quad (1a)$$

has customarily been calculated (see, for instance, the review by G. A. Corby, *Q.J.*, **80**, p. 491, 1954) by first working out the flow over corrugations

$$\zeta_1 = \cos kx \text{ at } z = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

This involves solving a differential equation for ζ, the vertical displacement of the particle at (x, z), of the form

$$\frac{\partial^2 \zeta}{\partial z^2} + (l^2 - k^2)\,\zeta = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

and if l is independent of z this has solutions

$$\zeta = A \cos \mu z + B \sin \mu z \quad . \quad . \quad . \quad . \quad (4)$$

where

$$\mu = +(l^2 - k^2)^{\frac{1}{2}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

so that for the case (2), when $l > k$,

$$\zeta = \cos kx \cos \mu z + B \cos kx \sin \mu z \quad . \quad . \quad . \quad . \quad (6)$$

in which B is *any* function of k, because $\sin \mu z = 0$ at the boundary. For the ridge (1), in which for the sake of example we will put $F(k) = e^{-kb}$, the displacement of the streamlines is given by

$$\zeta = \int_0^l \{e^{-kb} \cos kx \cos \mu z + B \cos kx \sin \mu z\} \, dk$$

$$+ \int_l^\infty \{e^{-kb} \cos kx \, e^{-\mu' z}\} \, dk \quad . \quad . \quad . \quad . \quad . \quad (7)$$

where $\mu' = +(k^2 - l^2)^{\frac{1}{2}}$, the term in $e^{+\mu' z}$ being rejected.

The value of this integral has usually been found approximately by assuming that for small k, $\mu = l$; and that for large k, e^{-kb} is negligible so that

$$\zeta = \cos lz \int_0^\infty e^{-kb} \cos kx \, dk + B_1(x) \sin lz \quad . \quad . \quad . \quad (8)$$

where $B_1(x)$ is *an arbitrary function of* x (derived from B). The approximation is good except when b is small, or when x is small and z is large.

The flow over the ridge

$$\zeta_1 = \frac{a^2 b}{b^2 + x^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

is therefore

$$\zeta = \frac{a^2 b}{b^2 + x^2} \cos lz + B_1(x) \sin lz \quad . \quad . \quad . \quad . \quad (10)$$

It has been customary to use the form (1*a*) and instead of (6) to arrive at the equation

$$\zeta = e^{i(kx \pm \mu z)} \text{ (real part)} \quad . \quad . \quad . \quad . \quad . \quad (11)$$

whence for flow over the ridge (9) we get

$$\zeta = \frac{a^2 (b \cos lz \pm x \sin lz)}{b^2 + x^2} \quad . \quad . \quad . \quad . \quad . \quad (12)$$

But Eq. (11) is incorrect because

$$\cos \mu z \times \text{real part of } e^{ikx} \neq \text{real part of } e^{i(kx \pm \mu z)} \quad . \quad . \quad (13)$$

In arriving at (12) we have indeed obtained a solution of the problem by taking for $B_1(x)$ the function $a^2 x/(b^2 + x^2)$. This arises solely because it is the imaginary part of (1a) and has nothing to do with the physical problem.

Clearly any wave motion, however arbitrary in form, which is zero at the ground can be added to any wave motion for a given mountain, but in order to obtain a practical solution these waves must be ignored unless a second boundary condition such as a rigid lid requires their presence. It is wrong to assume that the two solutions in (12) are the *only* two which can be used to satisfy any boundary condition which may be imposed.

In the absence of a special second condition waves corresponding to no disturbance at the ground should be excluded for the same reason that lee waves on the upstream side of the mountain are excluded. This is equivalent to saying that all waves are excluded which are not directly attributable to the mountain. On account of the erroneous way in which (12) was arrived at it appeared that the special solution $\zeta = a^2 x \sin lz/(b^2 + x^2)$ was related directly to the amplitude of the mountain, and was therefore attributable to it. The solution can never be unique in the mathematical sense, because any waves whose amplitude is zero at the ground can always be added.

One of the most important consequences of this is that the flow above a mountain is approximately represented by $\cos lz$ times the mountain shape, together with any lee waves from mountains upstream and the mountain itself. Since the variation with height of the amplitude of the lee waves is different from $\cos lz$, a combination of the direct effect of the mountain and the lee waves may be to produce a flow in which the wave crests at one altitude are not directly beneath those at a higher altitude. But the crests of all the separate components of the whole pattern lie in vertical planes. The appearance of crests lying in planes that are not vertical may be more pronounced if there are two or more possible lee-wave lengths in the airstream, because these must have markedly different variations in amplitude with height.

Another aspect of this method of determining the solution will be discussed in a paper on large-amplitude waves which Mr. H. E. Klieforth and I are at present preparing.

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Exhibition Road,
London, S.W.7.
26 October 1957.

551.577.21 (41/2) : 551.577.3

Annual rainfall probability maps of the British Isles

By G. Reynolds

I have read with considerable interest S. Gregory's recent paper (*Q.J.*, **83**, p. 543). These maps fulfil a great need. It is a pity that the author was satisfied that the frequency distribution of annual rainfall could be expressed by using only two parameters, mean and standard deviation.

It is generally recognized that annual rainfall statistics in Britain are positively skew, and failure to take this factor into account reduces the value of the resultant maps. Whilst it must be conceded that such a small scale can only depict broad outlines, a systematic error of this nature is regrettable.

The following figures, relating to annual rainfall at Bidston, illustrate the effect of skewness.

During the period selected by Mr. Gregory for analysis (1901-30) the rainfall of only one year was below average by more than 5 in. while three years had more than 5 in. in excess of average. That this is not purely a phenomenon of this particular period is demonstrated by figures relating to the full period of the record, 1867—1956. The greatest annual rainfall has been 17 in. above average, whilst the driest year was 8·6 in. below average. The modal rainfall is about 26 in. while the mean is 28·61 in. Rainfall in excess of 34 in. (5·4 in. above average) is more frequent than annual totals of less than 24 in. (4·6 in. below average).

Thirty years' record is perhaps a short period from which to compute reliable estimates of the degree of skewness, but smoothing techniques could be applied and a zonal correction determined. I hope Mr. Gregory will feel disposed to elaborate his method and publish revised maps if the corrections justify this at a later date. There seems to me to be a case for the publication of his statistical parameters also, for the benefit of water engineers. Means can, of course, be obtained, but standard deviations and skewness parameters are not, so far as I know, obtainable.

North of Scotland Hydro-Electric Board,
Rothesay Terrace,
Edinburgh 3.
28 November 1957.

Reply

By S. Gregory

Whilst agreeing with Mr. Reynolds that a tendency for positive skewness is frequently apparent in annual rainfall conditions in the British Isles, I feel that he somewhat misstates the position especially in the context of my recent paper. If the aim of this paper had been to present data for the calculation of probabilities at a series of stations, then obviously the skewness would have had to be included. The value of such a study would be limited, however, for it would merely provide information for a few specific sites – the water engineer would not be interested in many of them, but I am sure that he is quite capable of calculating the mean, standard deviation and skewness parameters himself for those localities in which he is interested.

The actual aim of the paper, however, was to present a general overall picture of the distribution of rainfall probability, and the methods used were determined by this fact. On the scale at which the maps were to be reproduced, and indeed on the original maps themselves, the inclusion of skewness would have made little significant difference, for skewness is nowhere large. Coupled with this is the fact that the skewness parameter varies from station to station, and that to consider it objectively and systematically for the 670 ' derived stations ' which were used (and without which a satisfactory distribution pattern could not have been defined) is not really possible.

Furthermore, Mr. Reynolds's assumption that the failure to include the skewness parameter necessarily leads to error, and moreover that this error is a systematic one, is not entirely correct. In the following Table the actual and calculated values are compared for five stations; one of them is Bidston, but using the data published in *British Rainfall* and not the Observatory's own records (*Q.J.*, **79**, p. 294, 1953), whilst the other four are stations which were presented in an earlier paper (*Geogr. J.*, p. 346, 1956) where they were chosen as representative examples of four different types of annual rainfall trend. These five stations would seem, therefore, to provide a reasonable cross-section of possible conditions.

TABLE 1. NUMBER OF YEARS WITH LESS THAN CERTAIN AMOUNTS OF RAINFALL
(a = actual number; b = calculated number)

	20 in.		30 in.		40 in.		50 in.	
A (1901-30)	a	b	a	b	a	b	a	b
Birkenhead (Bidston)	0	0	21	20	30	30	30	30
Oxford (Magdalen)	4	4·5	26	26	30	30	30	30
Batheaston Res.	0	0	8	7·5	23	24	30	30
Wigan W.W. (Worthington)	0	0	2	1·5	20	18	30	29·5
Loch Finlas	0	0	0	0	0	0·5	5	4
B (1881-1950)								
Birkenhead (Bidston)	1	1	49	47	70	70	70	70
Oxford (Magdalen)	13	11	64	61	70	70	70	70
Batheaston Res.	0	1	27	18	62	56	70	70
Wigan W.W. (Worthington)	0	0	6	4	49	42	70	69
Loch Finlas (1885-1950)	0	0	0	0	2	1	13	9

As can be seen in A of Table 1, the formula used does not necessarily overestimate the frequency of lower rainfall conditions (as Mr. Reynolds implies), for the differences which do occur are in both directions, and are invariably small. Of far greater relevance is the question of whether 30 years is adequate for the computation of skewness, especially in the light of the relationship of the 1901-30 period to the full observational period. A comparison of A and B of Table 1 will indicate the validity of my comments in the original paper concerning the tendency for the 1901-30 figures to underestimate the frequency of drier conditions over a longer period, whilst any correction of the 1901-30 data for slight positive skewness would, in fact, increase these discrepancies. What would seem desirable, therefore, is an analysis of records over a long period (with or without the skewness parameter), but this depends upon the existence of a sufficiently large and well-distributed number of stations with long records, and a detailed rainfall distribution map for the same period so that ' derived stations ' can be obtained for the areas devoid of stations. Can the Meteorological Office oblige?

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551.557.5 : 551.513.1 : 608.4

Jet stream and long waves in a steady rotating-dishpan experiment : structure of the circulation

By HERBERT RIEHL and DAVE FULTZ

(Read 20 November 1957. See *Q.J.*, **83**, p. 215)

Prof. P. A. SHEPPARD (*President*) : The authors remark (pp. 222-3) on the existence of surface easterlies in the model, the whole bottom being a source of westerly momentum, with the vertical walls providing the necessary sink of momentum. The distribution of momentum sources and sinks is notably different in the atmosphere, but the authors suggest that the similarity of flow in the bulk of fluid in the model and the atmosphere implies that the general pattern is hardly to be interpreted in terms of momentum balance. I think there is some truth in this inference in the sense that the bulk of the flow must be determined by the distribution of heat sources and sinks and by the rotation, and that the 'boundary layer' flows will provide the necessary adjustment between the bulk flow and the boundaries. But any understanding (' insight ' is the authors' word) of the general circulation in either system should involve an appreciation of how the momentum balance is achieved throughout each system. I understand the problem of the general circulation to be to give a consistent account of the whole field of motion as it exists under a given radiation input and rate of rotation, and to be able to predict the effect of small variations in the parameters concerned (solar constant, atmospheric composition, etc.). We are still some way from the latter goal and at this stage I would not turn a blind eye to momentum transfer in the system.

Dr. R. S. SCORER : Since the heating and cooling is at vertical surfaces, except possibly for friction and a bit of conduction through the bottom, the system is in a sense antisymmetrical with respect to a point in the centre of a longitudinal (radial) vertical section. This means that the motion in the upper (or lower) half could be deduced (by mirrors) if the motion in the lower (or upper) half were known. This is not so in the atmosphere where the bottom provides sources and sinks of heat and angular momentum, and there are no vertical surfaces doing this. Thus the atmosphere cares which way up it is but the model does not, in the sense that nothing new would be obtained if a lid were placed at the free surface and gravity were reversed. Could Dr. Fultz say how this affects comparisons between the model and the atmosphere ?

Mr. J. S. SAWYER : The flow at the top surface of the dishpan contains a 'jet stream' and is quite different from the flow at the bottom of the pan. The only essential factor which makes the system unsymmetrical about a horizontal plane through the middle level of the pan is the existence of friction at the bottom and not at the top of the pan. Can we deduce that bottom friction is an essential factor in setting up the 'jet-stream' in the dishpan and in the atmosphere ?

Mr. A. H. GORDON: The Coriolis Force in this experiment is constant since the dishpan rotates at constant angular velocity. But suppose it were made to vary. Suppose an apparatus could be constructed in which the Coriolis Force varies, in the same way as it does, in fact, upon the rotating earth. What then would be the effect upon the observed water circulation ? Would there be big differences from the picture described this evening ? In particular would the observed variation be more like that observed in the real atmosphere than the picture described this evening ?

Dr. D. FULTZ (*in reply, on behalf of the authors*): I think we can quite agree with Professor Sheppard that any real understanding of the general circulation must involve an appreciation of how momentum balance is achieved under given conditions. Our remark to which he refers and additional discussions in a succeeding paper are intended mainly to point out the dangers of too hasty generalization from such a general principle, using atmospheric experience alone and in the absence of sufficiently detailed and unequivocal theoretical models. For example, Jeffreys's conclusion that, given the surface wind distribution, eddies with certain momentum transfer properties must be present, certainly follows. But, if anyone were tempted to conclude that some alteration in the boundary conditions which might be thought to bring surface easterlies in middle latitudes would thereby completely change the upper eddy structure, the experiment shows that in a number of important respects, though definitely not all, the structure could remain essentially the same.

Since, unless I have made an error, the equations of motion are not invariant to reflections through the midpoint there is no compelling *a priori* reason for expecting the solutions to have the symmetry Dr. Scorer suggests. Admitting the qualitative differences of symmetry, I can see reasons both for regarding the experiment as responsive to friction at the bottom (and lacking symmetry for that reason) and for seeing more symmetry of this type in the atmosphere than might at first glance appear. With respect to the first, some related experiments in a large hemisphere have shown that one can have at very low Rossby numbers a similar interior distribution with positive thermal wind and at times waves either with westerlies (maximum at top) or with easterlies (maximum at bottom) almost everywhere. The difference was due to absence or presence of air torque at the top (the rotation was rather high at 10 r.p.m. or more). This certainly suggests that presence of the bottom can determine such things as the overall mean zonal current. Although the measured mean zonal components at the bottom in the present experiment are all easterly, the variations with radius are such as to look quite like a surface wind profile displaced toward easterlies except in the extreme strength of the polar easterlies. I conclude that in this respect the experimental base is behaving more like the ground than at first appears. With respect to the second matter of heating effects, I think one has to regard the interior in the experiment as corresponding, for any fairly detailed comparisons, to a middle latitude strip where advection effects tend to dominate and short time-changes are quasi-adiabatic. The deep tropics then serve the wall function of transferring heat to considerable heights and the motion north of the sub-tropical ridge is, like the experiment, quasi-adiabatic and to a considerable extent independent of the source arrangement at the ground below.

Mr. Sawyer's question concerns a more subtle possibility of frictional influence. In some cases experimental jets give a good deal of the impression of separated boundary layers. I am inclined, however, to think that the jet is not essentially connected with friction although that possibility cannot be ruled out at present. A variety of types of experiment exhibit jets in such a way as to suggest that it is an inviscid phenomenon but all involve boundaries, different though they be, that might be a required part of the mechanism. I think it will be very much worth while to run some tests with as much variation in this boundary condition as possible (especially with as nearly a free boundary at the bottom as is feasible and with lids in contact at the top) in order to see what variations occur in the jet. It might then be possible to draw a more definite conclusion.

In answer to Mr. Gordon's question I may say that it is feasible to obtain at least some direct variation of the Coriolis parameter by using a container figured to the equilibrium paraboloid for some rather high rotation, say 60 r.p.m. The following very interesting difficulty has then been shown to arise by von Arx's oceanographic experiments : If the fluid is homogeneous and a thin layer is rotated at the equilibrium rate the nominal β-effect is exactly compensated by the variation in the perpendicular depth of the layer. In the case of the Gulf Stream problem, no westward intensification is obtained. A higher than equilibrium rate then has to be used in order to avoid this compensation and this can cause further difficulties when density variations occur. We made some trials of this kind long ago but the main difficulty with such high rotation rates and the corresponding size containers is to reach a high enough thermal Rossby number. We got rather unrealistic results. The experiment is perfectly feasible, however, and when done at the proper thermal Rossby numbers might have some advantages. I do not think there would be any large qualitative differences. The likeliest effect would be to make wave propagation rates less rapid toward the east and more realistic, but not to change the structures very much.

551.513.2 : 551.552

Evidence of a mean meridional circulation in the atmosphere from surface wind observations

By G. B. TUCKER

(Read 20 November 1957. See *Q.J.*, **83**, p. 290)

Dr. R. S. Scorer : Dr. Tucker, in his presentation, said that the mean value of $-\frac{1}{\rho}\frac{\partial p}{\partial x}$ round a line of latitude was zero. Since equatorward winds are, on the average, cooler than poleward winds it is likely that this is not correct. Could Dr. Tucker indicate the magnitude of the error ?

Dr. A. G. Forsdyke : Can Dr. Tucker explain why, in the parts of Fig. 2 relating to meridional components of wind, the magnitudes for the oceans and land and sea combined are approximately equal, at least in middle and low latitudes ? I would think that, owing to frictional effects, the oceanic components would everywhere be the greater, as indeed Fig. 2 indicates for the zonal components.

Mr. T. V. Davies : Eq. (2) on p. 300, namely

$$-\rho l\bar{v} = \frac{\partial \bar{\tau}_{xz}}{\partial z} + \frac{\partial \bar{\tau}_{xy}}{\partial y}$$

has been discussed for the zone 35°N to 45°N and has been approximated to read :

$$-\rho l\bar{v} = \frac{\partial \bar{\tau}_{xz}}{\partial z}$$

Does the same approximation hold good in zones near the equator ?

Dr. G. B. Tucker (*in reply*) : My neglect of the correlation of wind direction with temperature, to which Dr. Scorer refers, has little effect on the calculations. For example, a large mean value of $\frac{1}{\rho}\frac{\partial p}{\partial x}$ would be 2×10^{-4} cm sec^{-2}; the contribution of this to $\frac{\partial \bar{\tau}_{xz}}{\partial z}$ is about 2×10^{-3} dynes cm^{-2} (100 m)$^{-1}$ which is generally less than 5 per cent of values given in Table 5.

Without becoming involved in too much detail, Dr. Forsdyke's point can be explained by considering the effect of surface friction on a geostrophic south-westerly wind of equal magnitude over land and sea. Over the sea, surface friction causes the surface wind to blow at an angle to the isobars with a speed less than geostrophic, resulting in an increased southerly component and a decreased westerly component. Over the land, surface friction is greater and this will result in both the angular deflection and the decrease in speed being greater than over the sea; the net result is a southerly component nearly equal to that over sea but a westerly component substantially less than formerly. Similar effects occur with all wind directions. Notwithstanding the unequal distribution of land and sea, the overall effect in time and around a latitude circle is the approximate equality of mean meridional velocity over land and sea in middle latitudes but a greater mean zonal velocity over the sea than over the land.

In reply to Mr. Davies, in very low latitudes where the term $-\rho l\bar{v}$ is very small it becomes comparable with the inertia terms which were neglected in deriving Eq. (2) and with $\frac{\partial \bar{\tau}_{xy}}{\partial y}$. The approximate Eq. (3) therefore does not hold good for very low latitudes; however, there seems little doubt that $\frac{\partial \bar{\tau}_{xz}}{\partial z}$ is small in these regions and that upward transport of momentum is maintained—at least in the lower troposphere.

551.574.41 : 551.584.42 : 551.521.2

Dew

By J. L. MONTEITH

(Read 18 December 1957. See *Q.J.*, **83**, p. 322)

Mr. I. F. Long : From his records Dr. Monteith concludes that dew may form on a grass cover under conditions when the vapour pressure gradient is negative both within the grass and above the grass. This is in conflict with my own experience on other crops (long grass, potatoes, brussels sprouts, wheat), for on no occasion have I ever visually observed dew formation until an inversion of vapour pressure gradient occurred above the crop, nor was it ever recorded in potatoes by Hirst's dew balance (Hirst, *Q.J.*, **80**, p. 227, 1954) in the summers of 1953-56 until there was such an inversion.

I suggest that the ' dew ' absorbed by the filter paper under these conditions may have

een liquid exuded from the grass itself, a process known as guttation (Long, *Weather*, **10**, p. 128, 955). Guttation from grass occurs throughout most of the year and particularly in May when ew growth is starting. Exudation usually begins at sunset when transpiration through the tomata ceases, a root pressure builds up and water is exuded at the tips of the grass leaves. Varm, moist soil is also an asset to guttation.

The conditions for guttation at Harlington in the period 6-12 May 1954 (Monteith, Table 6) ere ideal. In the four days before, there were 10 mm of rain and only nine hours sunshine, e. little evaporation, so that the surface would be moist, as it was at Rothamsted where similar onditions existed. Here the guttation was so marked that it was photographed.

Mr. J. Lorch: For many years now dew has been almost the exclusive province of otanists and dilettantes, so both dew and Dr. Monteith should be congratulated on this new pproach and on the excellent equipment, which provides for the first time the means for answering ome old questions.

I would like to ask whether you think hairiness of a grass leaf might affect the formation of lew? It does seem likely that it affects the convection currents close to the leaf surface, also ave you made any measurements on bare soil?

Mr. R. G. Veryard: Whether the condensed water consists of 'dewfall,' is the result f the distillation process, or both, it is presumably all lost in the morning as the result of evapora- ion. As the distilled water may exceed the dewfall there must be a net loss of moisture from the oil. Thus after calm clear nights, contrary to old beliefs, the soil has become drier. Does Dr. Monteith agree, or is there a reverse process to that of distillation? It would be interesting o know what happens in the case of bare soil.

Mr. C. E. Wallington: Could Dr. Monteith elaborate on his statement that there ppeared to be no change of rate of fall of temperature with the dew formation. Does this mean hat such changes were not normally detected or that they did occur but were not closely ssociated with the onset of dew deposition?

Mr. E. Gold: Do Dr. Monteith's investigations enable him to give a satisfactory xplanation of the not infrequent marked deposition of dew on the flat top of a thermometer creen (about 5 ft above ground level) on nights when there is no dew on stone paths and not nuch on the grass?

Mr. L. C. W. Bonacina: I think it not unlikely that Mr. Gold's finding of more dew n the top of his thermometer screen than on the ground could be attributed to light wind.

There are, of course, two processes involved in dew formation: (1) radiation, the more amiliar process in lowland country, (2) impact of a damp wind against a cold surface, a process f considerable importance in mountainous country.

Mr. J. M. Craddock: The publication of Dr. Monteith's paper is a matter of great atisfaction to me, personally. About 1950 I reached the view that the best way to study dewfall vas to maintain a continuous record of the weight of a piece of the natural ground surface, eparated from its surroundings, but otherwise disturbed as little as possible, and in 1951 I escribed a prototype apparatus and showed the feasability of measuring the dewfall on a cover f small lettuce plants. Circumstances prevented me following the matter up myself, and I am ery pleased to see that Dr. Monteith has pursued the same line and carried out the present xcellent investigation.

I hope that now the technical difficulties have been overcome the recording balance will be sed to investigate dewfall on other plants providing ground cover, such as cereals, cabbages r melon plants, and if possible on small trees.

Dr. J. L. Monteith (*in reply*): I agree with Mr. Long that a vapour-pressure inversion nvariably develops on a dewy night, but whether the gradient changes sign at exactly the moment vhen dew begins to form is another matter. As I have explained in the paper, the *precise* time f dew formation cannot be determined by a balance of the type used at Harlington, nor can it e determined by Hirst's dew balance in which the vegetation is unnaturally separated from the oil. In his visual observations Mr. Long may have missed the rather rare nights on which dew ormation (by distillation) may have preceded the inversion for a short period – the transition égime. It is possible, however, that over crops such as potatoes and wheat on which dewfall s likely to be more important than on short grass, the development of a vapour-pressure inversion nd the formation of dew may always be simultaneous.

Guttation *was* observed at Harlington in May 1954, but the observations for this period ere used in heat-balance calculations only. The distillation data of Table 3 were obtained n nights when dew took the form of minute globules on the surface of the leaves and there vas no sign of large droplets on the tips of the blades which are characteristic of guttation.

In reply to Mr. Lorch, the hairiness of a leaf could conceivably determine the amount of dew which it could retain before run-off occurred, and it might assist the absorption of dew as Lehmann and Schanderl have suggested. But dewfall and distillation on a leaf are controlled primarily by its energy balance; and I believe that as this is dominated by the thermal properties of the environment the influence of surface geometry will be negligible. No measurements were made on bare soil, since it was impossible to install the balance in fallow land.

After sunrise, distillation is lost by evaporation as Mr. Veryard suggests, but in the absence of distillation approximately the same amount of evaporation would take place by stomatal transpiration. The overall loss of soil moisture is therefore the same whether distillation occurs or not. Calm clear nights conserve soil moisture by distillation : it is in cloudy, windy weather when evaporation continues after sunset that the soil becomes drier overnight.

Reverse distillation may take place in the late afternoon when the leaves (in sunshine) are warmer than the topsoil (shaded by the leaves when the sun is low). A considerable proportion of the moisture required for subsequent distillation may be taken up in this way.

The discontinuities in the nocturnal cooling rate to which Mr. Wallington refers were frequently observed on a thermograph record, but since they occurred from two hours before to two hours after the onset of condensation, I cannot believe that there is any connection between the two.

The tops of thermometer screens, the roofs of cars, etc., are thermally insulated from the ground. They therefore cool more rapidly below the dew-point than objects in contact with the ground and become covered more rapidly with dew or hoar frost, as Mr. Gold has observed. The way in which dew formation on a given surface varies with the height of the surface is a complicated function of wind and humidity gradient. Contrary to recent claims, the relative importance of dewfall and distillation cannot be assessed from such observations.

Mr. Craddock may be interested to know that a much larger balance is now being used at Rothamsted to measure condensation and evaporation.

551.573 : 551.586 : 631

Water losses from various land surfaces

By N. E. RIDER

(Read 18 December 1957. See *Q.J.*, **83**, p. 181)

Dr. H. L. Penman : Mr. Rider's paper presents us with a clash of faiths. On the basis of what I regard as valid physical reasoning, supported by some world-wide evidence, I believe that the rates of water loss from extensive crop covers exposed to the same weather are substantially equal, provided there is an adequate water supply available to meet the weather-imposed demand. Mr. Rider believes that the equation he uses tells the whole truth about upward water vapour transport, but has no field evidence to support his evaporation estimates. This field evidence is very difficult to obtain, particularly on the short time scale under discussion, but as we now have equipment at Rothamsted which gives a continuous record of transpiration rates we hope that Mr. Rider will be able to run his apparatus alongside ours next summer.

In the meantime we can only argue about possibilities, and I'd like to confine my comments to the exceptional day represented in Fig. 4. Here, at about 4 p.m. on the afternoon of 18 June, the rate for the peas was about 40×10^{-6} and for the grass about 3×10^{-6} g cm^{-2} sec^{-1}. On the basis of an energy balance the expected rate is of the order of 10×10^{-6} g cm^{-2} sec^{-1}. So if the Rider estimates are correct, the queries are : where are the peas getting all the extra energy from ? What are the alternative sinks for the energy surplus to evaporation requirement of the grass ? If the Rider estimates are not correct, is there a source of error that could affect the two crops in opposite senses ? I think there may be. Conceding that the evaporation rate from the peas *is* greater than from the grass, the air temperature over the grass will be higher than over the peas, and hence there will be different possibilities for free convection, and bulk transport of water vapour. A rough calculation, based on very simple assumptions, indicates that an upward air movement over the grass, and a downward air movement over the peas, both of the order of about 10 cm sec^{-1}, would bring the estimated transfers to about the same value near 10×10^{-6} g cm cm^{-2} sec^{-1}.

The situation here might well be very like what Priestley finds with heat transport, namely that the rate is dictated by radiation supply, and the gradients and transport mechanisms simply adjust themselves to fit circumstances over which they have no control.

(*Added later*) : I would like to make coherent some of my contributions to the staccato exchanges later in the discussion. For this particular piece of work the only field test is in the original measurements of Pasquill using small shallow turves in pots, and I've never been completely satisfied that the cutting of the root system had no effect whatever on transpiration rates. After this biological doubt – which I am not in a position to dispel – comes another, largely physical, provoked by Makkink's similar work in Holland in which he found that the loss from a turf varied greatly, depending on whether the grass was a little taller or a little shorter than its surroundings. But over-riding these is the very obvious fact that in all these evaporation studies – Pasquill's, Makkink's, the Lake Hefner work, and my own, to name only those I know best – the scatter in the plotting of estimated evaporation (from this or similar aerodynamic equations) against measured evaporation is enormous. Even after a very generous allowance for experimental error the scatter remains, and with it the query : is this treatment telling the whole story ? I very much want the answer to be " yes," because I want to use the equation in my own work, but I want to be able to use it confidently, and my confidence, never very great, is shaken by the last diagram in Fig. 4. I cannot believe that in the mid-afternoon of an English June day a healthy crop of grass was transpiring at a rate as low as 3×10^{-6} g cm^{-2} sec^{-1}, nor that any crop could transpire at a rate as high as 40×10^{-6} g cm^{-2} sec^{-1}. The grass result might, of course, be dismissed as non-representative because the field had been grazed until the day before, and it is conceivable that defoliation, fouling, or shock closure of stomata had either removed the effective transpiring surfaces, or sealed them.

The estimate for the peas, however, presents the challenge in energy balance already stated. The suggested solution is a supply of heat by advection, but however great the horizontal temperature gradient that may have existed the actual transfer to the crop demands a gradient of temperature downward from air to crop. The only evidence we have is that over the peas, until 19 hr, the gradient between 75 and 100 cm was directed *upward*, so that unless there was a dramatic change of sign of the temperature gradient just above the crop the peas were acting as a source of sensible heat for the atmosphere, and not as a sink. So my query remains : where did the extra energy come from ? My own answer is that it was never there, and that the estimate is an over-estimate.

This may well be a freak result, but it could be an extreme example of a general discrepancy, in which case some of the differences in Table 1 may be somewhat exaggerated.

Mr. E. Gold : In the case of the peas were the heights measured from soil level or from the average level of the upper surface of the crop ? For peas this is usually a very irregular upper surface.

Dr. G. Stanhill : Mr. Rider states in his paper that it is necessary to produce evidence that the crops considered were transpiring at their potential rate during the period covered by the observations. If this was not the case the differences in water loss might well have been caused by differences in soil-moisture deficit, depth of root zone or stomatal behaviour.

I believe that there is evidence from his measurements that the water loss from at least one of the crops was well below the potential rate. The calculated value of water loss for the seven comparable days given in Table 1 was 1·10 in. from the pea crop and 0·46 in. from the grass, whilst the potential weekly water loss estimated for Cambridgeshire during this period by Dr. Penman's method was 1·00 in. This suggests that whilst the pea crop was transpiring at the maximum potential rate the water loss from the grass was limited to half this rate. The soil-moisture deficit over this period varied from 2·5 to 5·0 in. (Table 3), such deficits would exhaust all the available soil moisture within the top 18 to 30 in. of an average soil and this might well be expected to limit transpiration. It is reasonable to suppose that the soil-moisture deficit under the pea crop was smaller than that under the grass because of the later development of full ground cover of transpiring vegetation in the pea crop.

Dr. J. L. Monteith (*partly communicated*) : Mr. Rider has commented on the peculiar energy balance which prevailed over the peas on the afternoon of 18 June and there is an aspect of this which I find rather disturbing. The net radiant energy available for evaporation and for heating the soil and the air is unlikely to have exceeded 25 mw cm^{-2}. The energy required for evaporation was 100 mw cm^{-2}; a positive amount of energy was required for heating the air (temperature decreased with height); and the energy for soil heating can be ignored. This means that *at least* 75 mw cm^{-2} would need to be supplied by horizontal advection, implying a very strong horizontal temperature gradient. It is unlikely that such a temperature gradient could exist without an associated horizontal gradient of vapour pressure. But Rider's Eq. (1) is relevant only to horizontally uniform conditions and its use is therefore inconsistent with the values of evaporation computed from it.

A further possible internal inconsistency is the variation between 18 and 19 June in the ratio $u_2 - u_1$ (peas)/$u_2 - u_1$ (grass). For a fully developed wind profile, $u_2 - u_1$ is proportional to the friction velocity in neutral conditions, and one would therefore expect to find that the ratio of velocity differences was roughly independent of wind speed for periods when there was no sensible change in the roughness of either crop. Fig. 4 shows that the ratio was about 1·2-1·4 from 16 hr to 20 hr on 18 June, that it changed sign for a period during the night and then assumed a value of about 2, and that on the 19th it was approximately 1·4-1·6. Can these variations be attributed entirely to variations in atmospheric stability or are they an indication that fetch was not always adequate ? Could Mr. Rider say what the fetch was and whether any measurements were made of horizontal gradients of wind, temperature and humidity downwind from the leading edge of the crop ?

I accept the accuracy of Mr. Rider's observations without question because I have the greatest admiration for the care which he has taken to eliminate every possible source of error. Nor would I presume to question the validity of Eq. (1) in horizontally uniform conditions. But without evidence that such uniformity was in fact achieved, Mr. Rider's paper fails to convince me that different crops, exposed to the same weather, can have transpiration rates differing by as much as an order of magnitude when there is no shortage of soil moisture. There is a great deal of sound evidence to the contrary which we cannot ignore.

Dr. J. V. Lake : I should like to draw attention to the importance of this controversy to horticulturists. Winter (*J. R. Horti. Soc.*, **82**, p. 476, 1957) has stated that under ordinary market-garden conditions vegetable crops only cover 21 per cent of the ground during the months from April to September so that the conditions somewhat resemble those investigated by Mr. Rider where a field is analogous to an oasis. Market gardening is an application where accurate irrigation can be very profitable and it is therefore important that an accurate means of calculating requirement in small fields should be found as soon as possible.

Mr. N. E. Rider (*in reply, partly communicated*) : Firstly may I make a general comment on the remarks made by Dr. Penman and Dr. Monteith. They have both selected the observations of 18 June for special attention, and I deliberately invited this by providing full details of the results for that day for which there is no obvious explanation. Here I must confess that I was tempted to reject these observations as due to some instrumental failure but could find no valid reason for doing so. The results for this period are quite exceptional; the highest rate of evaporation experienced throughout the remainder of the observations was 17×10^{-6} g cm^{-2} sec^{-1}, and although I have not given details of the more usual run of observations I hope they will accept my assurance that those were not impossible to explain from an energy-balance point of view. Dr. Penman himself provides us with a possible explanation for 18 June. We can see from the details given that the temperature over the grass was about 2°F higher than that over the peas during the hours in question. This, by itself, suggests a different partition of the available energy between air heating and the evaporative process for the two surfaces. There still remains the difficulty of the lapse of temperature above the peas which appears to prohibit the direct supply of energy from the air when the rate of evaporation required an energy input of about 100 mw cm^{-2}, but I cannot agree that a dramatic change of sign was required. The mean lapse over the peas was about 0·18°F and during the next hour when the evaporation was continuing at the high rate of 20×10^{-6} g cm^{-2} sec^{-1} this was replaced by inversion of almost identical magnitude. The corresponding lapses over the grass were 1·05 and 0·71°F, although not between strictly comparable heights. The dramatic differences lie there. The heat-balance picture may not have been quite as black as Dr. Monteith paints it. It seems likely that the soil and possibly the entrapped air within the dense mat of vegetation were providing some heat, rather than using it, during the hour. Some years ago Pasquill showed that, if one uses mean values of temperature difference, wind-speed difference, etc., in aerodynamic equations and the time over which the mean is taken is too long then the computed value of evaporation may be in error by a large amount. He was comparing hourly summations of evaporation for 24 hours with those calculated from the mean daily values of the meteorological elements. It may be that during periods of exceptional variation in the latter a period of an hour is too long a time over which to use mean values in aerodynamic equations. It is perhaps worth mentioning that the assumption of equal potential transpiration demands that, at any time, the division of the available energy between air heating and evaporation must be the same for different surfaces. In other words the Bowen ratio or, more simply, the ratio of the temperature to the humidity gradient must be identical over the surfaces under consideration. A glance at the detailed observations shows that this is not generally so. Further, the consumption of energy in evaporation several times that immediately being supplied directly from the sun has been noted recently by Lemon, Glaser and Satterwhite

(*Soil Sci. Soc. Amer. Proc.*, **21**, p. 464, 1957). They report a case, which was not isolated, in which the energy used was over twice that currently being supplied by radiation and their site of observation was ten miles from the upwind edge of a continuous surface of healthy cotton. The observation quoted was in the late afternoon.

Dr. Penman refers to Pasquill's observations with small evaporimeters when an attempt was made to check the validity of the equation I have used. In that work all possible care was taken to investigate the influence of cutting the root system. Evaporimeters of different depths were used and a number of them were arranged in statistical design to arrive at a mean result so that point-to-point variations due to differences in grass length, etc., (of the type observed by Makkink) were smoothed. The mean hourly rate of evaporation, which did not apply to any particular point on the pasture, was compared with evaluations from the aerodynamic equation with satisfactory results. Penman also refers to the Lake Hefner results. As he says, the scatter in the points was large but it must be realised that, as is pointed out by the authors themselves, it would have been surprising if any other result had been obtained given the resolving power of the apparatus used.

Dr. Monteith asks about the fetch over the surfaces. I can only make the unsatisfactory reply that these were made as long as possible and were not as great as I should have liked to see. Owing to changes in wind direction they may have been as little as 50 m on occasions, particularly when the wind was light and variable, and I agree with him that this is the most unsatisfactory feature of the observational programme described. Given our weather with its almost hourly changes in wind direction and our farming methods which tend to result in a large number of small areas carrying different crops, it is extremely difficult to find a satisfactory site for work of this type. We should note that irrigation in this country is confined to small areas and, even if it is accepted that the information in the technical bulletin cited provides the correct information for calculating the date at which irrigation should commence, I think it must be accepted that subsequent to that date the water loss from the irrigated area will be greater than that suggested. The irrigated area will be surrounded by other areas which are short of water and there will be considerable advection of energy to the irrigated surface. Dr. Stanhill's remarks also suggest a reason for caution in applying the advice given.

Mr. Gold asks about the reference level for height measurement. In all cases this was the soil-surface level and the particular field of peas used presented an unusually dense and even cover over the ground.

551.574.1

The formation of large droplets in small cumulus

By B. J. MASON and D. K. GHOSH

(Read 15 January 1958. See *Q.J.*, **83**, p. 501)

Prof. P. A. Sheppard (*President*) : Is there positive evidence that there is not a natural supply of nuclei (not necessarily hygroscopic) of the required size and in the required number to produce precipitation from the model cloud used in this paper ?

Dr. A. W. Brewer : I would like to ask Dr. Mason whether he knows if rain which falls in India, formed by non-freezing processes, only occurs in areas where an adequate supply of sea-salt nuclei could be expected. Such rain has been described either privately or publicly by Sir George Simpson, Sir Charles Normand and Mr. R. G. Veryard, but I am not sure to what areas the descriptions applied.

Mr. E. Gold : I think rainfall at 5 mm/hr or even 1 mm/hr does not often go on for long in this country. Has there been any examination of the likelihood of rain of this character in the air in which cumulus cloud, of the type discussed, forms. May it not be that in this air there is not much ' washing out,' and enough maritime nuclei be left to make unnecessary the up-and-down motion of drop nuclei ?

Dr. R. C. Sutcliffe : Dr. Mason has modestly implied that he is not entirely happy about his results because the calculations are very tight and can only just provide for precipitation from the shallow clouds considered. It would appear to me that the results are very satisfactory indeed and give as much support for the physical ideas as could be expected from this type of calculation. One recalls that precipitation is not usual from such clouds and that it appears (at least this is my own impression) to occur more easily over the sea than inland, and, over the land, more easily in the early part of the morning than later in the day. All this could be reconciled

with Dr. Mason's arguments and one must expect the conditions to be rather critical; if not, we should expect shallow clouds to give showers either always or not at all. Perhaps Dr. Mason could explain why he has misgivings.

Mr. L. C. W. Bonacina: Regarding Dr. Sutcliffe's suggestion that there may be a greater likelihood of slight rain on the morning hours, I cannot speak definitely for *rain* but I have noticed a marked tendency in cold weather in south-east England for light snowfall from low thin stratus, about 8 or 9 a.m., dispersing as the morning wears on.

Dr. B. J. Mason (*in reply on behalf on the authors*): In answer to Professor Sheppard, I would say that there is very little *direct* evidence for a natural deficiency of nuclei for initiating showers by coalescence but that I believe there to be situations (e.g., after a prolonged spell of widespread rain, or when the air mass has had a long overland track) when the concentrations of large nuclei are very low. In maritime air masses, in which showers seem to fall rather readily from quite shallow cumulus, salt nuclei of $m > 10^{-10}$ g are found in concentrations of order 1/l-10/l, and these appear sufficient to give a light shower even if each nucleus grows only to drizzle-drop size. Unfortunately, there have been very few measurements of giant nuclei in continental air masses and at places remote from the sea. There is some evidence that, in the absence of appreciable precipitation, salt nuclei may be carried 100 km or more inland without their concentrations at cloud level being seriously reduced, but that the giant nuclei ($m > 10^{-11}$ g) are effectively washed out by widespread rain. On the other hand, in such places as New Mexico, cumulus may grow to impressive proportions without precipitating; this may be due to a shortage of nuclei or, alternatively, because the clouds are too short-lived or fail to satisfy other dynamical requirements.

As to whether *hygroscopic* nuclei are essential, I see no good reason why coalescence should not be initiated by large non-hygroscopic dust particles ($r > 20\,\mu$) providing their surfaces are wettable. Such particles are carried up to appreciable heights in dust storms, but since their terminal velocities exceed 5 cm/sec, their half-life in the atmosphere will not usually exceed 24 hours.

In reply to Mr. Gold, I calculate that giant nuclei are washed out by rain according to the formula

$$n = n_0 \exp\left[-4{\cdot}4 \times 10^{-4} E p^{0{\cdot}6} t\right],$$

where E is the collection efficiency of the raindrops for the particles and is not critically dependent on raindrop size, and p is the rate of rainfall in mm/hr. On this basis, the concentration of nuclei of $m = 10^{-10}$ g, assumed to exist as solution droplets in equilibrium with 80 per cent relative humidity, will be reduced by 90 per cent in only 1 hr if $p = 5$ mm/hr, and in 3 hr, if $p = 1$ mm/hr. This indicates that the large nuclei, with which we are concerned, will be effectively removed over a large area by prolonged widespread rain associated with an active depression. On the other hand, I agree with Mr. Gold that the formation of potential shower clouds in an air mass which has very recently been scavenged by widespread rain may not be the most usual occurrence, and that showers occurring in the cold air behind a depression, for example, may have an adequate source of nuclei.

We are pleased that Dr. Sutcliffe thinks the results of our calculations agree fairly well with observation. On the other hand, I think we should be careful not to claim too much for such a simple cloud model in which the dynamics and thermodynamics of the mixing processes cannot yet be properly represented. In maritime air masses, at least, sufficient giant nuclei are likely to be present to produce showers from the rather weak, shallow clouds we are considering. It is more difficult to account for the numbers of large droplets which are observed in these clouds. We have shown that the measured concentrations of drops with $r > 30\,\mu$ can be accounted or, but only just, by the measured concentrations of giant hygroscopic nuclei. However, droplets of $r \simeq 20\,\mu$ are found to exist in concentrations of 1/cm^3-10/cm^3, often near cloud base; these must grow almost entirely by condensation but they could not achieve this size on nuclei of $m = 10^{-12}$ g, present in these concentrations, during adiabatic ascent from cloud base. It may be that some droplets which commence growth in one bubble become carried down on its flanks, and later continue their growth in a second rising bubble.

In reply to Dr. Brewer, there are very few published data on the large-nucleus content of the air over India. Fournier d'Albe found that the concentrations of hygroscopic nuclei, as measured at the *surface* in Western Pakistan, fell off with distance from the coast and were about ten times higher in the Monsoon season with the wind blowing off the ocean. The only well-documented cases of non-freezing rain with which I am acquainted occurred in Madras during the north-east Monsoon, conditions in which there was probably an adequate supply of giant salt nuclei.

Proceedings at meetings of the Society

Ordinary Meeting, 15 January 1958

The following candidates were declared elected that day by the Council :

FELLOWS

ATKINSON, Ralph Stuart, F.R.A.S., 46 White Hill Avenue, Barnsley, Yorks.
GBECKOR-KOVE, Nicholas A., c/o Meteorological Dept., P.O. Box 744, Accra, Ghana.
GILL, Mancharan S., Malayan Meteorological Service, Fullerton Building, Singapore 1.
JACKSON, Michael, 15 Porlock Road, Flixton, Manchester, Lancs. (*transfer from Student Membership*).
LO, Paul Edward Su Sien, c/o Malayan Meteorological Service, Fullerton Building, Singapore 1.
NEWMARK, David Michael, B.A., B. Comm., 'Lee Ho,' Rowan Park Avenue, Blackrock, Co. Dublin, Eire (*transfer from Student Membership*).
MORTON, John Leslie, 44 The Lawns, Hatch End, Middx.
PALMER, Prof. Clarence E., Institute of Geophysics, University of California, Los Angeles 24, California, U.S.A.
SHARP, Frederick Arthur, B.Sc., The Garden House, Bisham, Marlow, Bucks.
TURNER, John Stewart, M.Sc., Ph.D., Fluid Motion Laboratory, Barton Airport, Manchester, Lancs.

FOREIGN MEMBERS

CHARLES, Bernard Nathan, B.S., M.S., 10413 Princess Jeanne NE, Albuquerque, New Mexico, U.S.A.
REED, Richard John, Sc.D., Department of Meteorology, University of Washington, Seattle 5, Washington, U.S.A.

CANADIAN BRANCH

FELLOW

BAYNTON, Harold W., B.A., 1431 Creal Crescent, Ann Arbor, Michigan, U.S.A.

The following papers were read and discussed :

1. 'The formation of large droplets in small cumulus' by B. J. Mason and D. K. Ghosh (*Q.J.*, **83**, p. 501).
2. 'Intracloud lightning discharges' by L. G. Smith (*Q.J.*, **83**, p. 103); presented by Dr. E. T. Pierce.
3. 'Preliminary discharge processes in lightning flashes to ground' by N. D. Clarence and D. J. Malan (*Q.J.*, **83**, p. 161); presented by Dr. T. W. Wormell

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Ordinary Meeting, 19 February 1958

The following candidates were declared elected that day by the Council :

FELLOWS

ADAMS, James Andrew, B.A., 'Post Office House,' Balnamore, Ballymoney, Co. Antrim, N. Ireland (*transfer from Student Membership*).
DURBIN, William Gordon, B.Sc., 3 All Saints Crescent, Fox Lane Estate, Farnborough, Hants.
FERNANDO, Ponnaidulage Felix, 'Wembley,' Moratuwa, Ceylon.
GILES, Brian Douglas, M.A., c/o Miss A. Woodley, 31 Cranleigh Gardens, Southall, Middx.
HALLETT, John, B.Sc., D.I.C., 719 Wells Road, Bristol 4, Somerset (*transfer from Student Membership*).
LANGHAM, Edward Joseph, B.Sc., 19 Glebe Gardens, New Malden, Surrey (*transfer from Student Membership*).
LIOW, Leng Kung, 255 Bukit Timah Road, Singapore 2.
MADDEVER, Richard Sheeres, B.A., Clarendon Laboratory, Parks Road, Oxford (*transfer from Student Membership*).
OGILVIE, John Alexander Blackhall, A.R.Ae.S., c/o London Caravan Co., Stirling Corner, Barnet By-Pass, Elstree, Herts.
PYKE, Robin Leonard, Flt. Lieut., 'Piskey,' Owls End, Bury, Ramsey, Huntingdon.
SMITH, Christopher Cyril, 149 Newark Road, North Hykeham, Lincoln (*transfer from Student Membership*).

SMITH, Frank Barry, Ph.D., 58 Campbell Road, Salisbury, Wilts. (*transfer from Student Membership*).
THOMAS, David Corin, M.Sc., 53 Wentworth Avenue, Ascot, Berks. (*transfer from Student Membership*).

STUDENT MEMBER

COURTIER, Geoffrey Martin, ' Denham,' Highwood Hill, London, N.W. 7.

FOREIGN MEMBERS

SIPPLE, Capt. Milton Henry, B.A., M.S., Westfield Bungalow, Medmenham, Nr. Marlow, Bucks.
SULTAN, Abdul-Ghani J., M.Sc., 25/8/18 Nassah, Adhamiah, Baghdad, Iraq.

CANADIAN BRANCH

FELLOWS

LANE, Robert Kenneth, B.Sc., 315 Bloor Street West, Toronto, 5, Ontario, Canada.
LANGILLE, Robert Carden, M.Sc., Ph.D., 30 Glencairn Avenue, Ottawa 1, Ontario, Canada.

The following papers were read and discussed :

1. ' The air flow over a ridge – the effects of the upper boundary and high-level conditions ' by G. A. Corby and J. S. Sawyer (see p. 25 of this volume).
2. ' A numerical study of the wavelength and amplitude of lee waves ' by C. E. Wallington and J. Portnall (see p. 38 of this volume).

Ordinary Meeting 19 March 1958

The following candidates were declared elected that day by the Council :

FELLOWS

BARNETT, Frank, 52 Bartholomew Road, Meir, Stoke-on-Trent, Staffs.
BIRCH, Flt. Lieut. Harry, ' Anchorage,' 19a Tynewydd Road, Rhyl, Flint.
DEAR, John Richard, ' Lynridge,' Tunmers End, Chalfont St. Peter, Gerrard's Cross, Bucks. (*transfer from Student Membership*).
GREEN, John Sydney Adcock, B.Sc., 18 Meek Street, Chelsea, London, S.W.10.
INGAMELLS, Kenneth Roy, 2 Klondyke Cottages, Kexby Lane, Nr. Gainsborough, Lincs. (*reinstatement, transfer from Student Membership*).
SREENIVASAN, Peruvemba Seshu-Aiyar, M.Sc., Meteorological Office, Poona 5, India.
STEWART, Kenneth Hope, M.A., Ph.D., 38 King Edward's Grove, Teddington, Middx.
WARDEN, Pahlan Cawasha Dorabji, 2 Jubilee Avenue, Whitton, Twickenham, Middx.

FOREIGN MEMBER

ROSSMAN, Fritz Otto, D.Ph., 1308 18th Street, Alamogordo, New Mexico, U.S.A.

ASSOCIATE MEMBER

REVELL, Clifford George, B.Sc., P.O. Box 19, Whitianga, New Zealand.

STUDENT MEMBERS

BULMAN, Peter Joseph, Vernmore, Barnards Green, Malvern, Worcs.
JACKSON, Mervyn Charles, 45 Moorthorne Crescent, Porthill, Newcastle, Staffs.
MILES, David, 41a Clare Road, Maidenhead, Berks.

The following papers were read and discussed :

1. ' Theoretical studies of diurnal wind-structure variations in the planetary boundary layer ' by K. Buajitti and A. K. Blackadar (*Q.J.*, **83**, p. 486); presented by Dr. D. R. Davies.
2. ' A synoptic study of West African disturbance lines ' by R. H. Eldridge (*Q.J.*, **83**, p. 303).

Reviews

Exploring the atmosphere's first mile. Eds. H. H. Lettau and B. Davidson. London (Pergamon Press), 1957. Vol. 1 : *Instrumentation and data evaluation*, pp. xiv, 376; 113 Figs.; 59 Tables. Vol. 2 : *Site description and data tabulation*, pp. v, 377-578, many Figures and Tables. £7.

The ' Great Plains turbulence field programme,' which is reported in these two volumes, grew from studies initiated in 1947 of the general background of information on the micrometeorology of the lower atmosphere. It was contended that much required to be done observationally to reach adequacy in ' accuracy, representativeness and detail ' and, as various organizations became independently actively engaged to this end, it was also contended that the fullest benefit of their efforts would only be obtained by combining their activities at one suitable site. The latter was found in the open spaces of Nebraska and a programme of intensive cooperative measurements was planned and carried out during the period 1 August to 8 September 1953. 15 university and government departments participated, supplying some 70 personnel, including many well-known U.S. workers in the field.

Vol. 1 opens with a short contribution from the editors, outlining the history of the programme and summarizing its achievements. The rest of the volume deals with the instruments, the techniques and some of the results, in a series of 65 separate papers, most of which are very short, by 34 authors. Three of the papers near the end, by one of the editors, are devoted to some general discussion of the results and occupy approximately 70 of the 376 pages. Of the remainder, 16 contributions deal with soil properties, 15 with low-level vertical profiles (including those of ozone) and 10 with fluctuations of temperature and wind. Individual or smaller groups of contributions are concerned with synoptic aspects, radiation, surface drag, dew and observations of wind, temperature and humidity in the ' free air.' In the latter connexion the title of the work might be thought to be somewhat biased, for only about 30 pages are in fact devoted to measurements above about 50 ft. The programme was carried out with intensive concentration on seven ' general observation periods ' averaging 25 hr each in duration, with the observations at set 2-hr intervals. Summaries of the data, in a standard system of tables, are given for each of the periods in turn in Vol. 2, which also contains relevant details about the site, the climatology and the scheduling of the observations.

A full assessment of the value of a project such as this is clearly a matter for the test of time. The present reviewer's immediate reactions are somewhat conflicting. On the one hand the obvious achievement in field organization and the sheer quantity of numerical data command admiration, and there is no doubt considerable value yet to be extracted from the statistics. On the other hand, as reading proceeded, it was impossible to avoid a growing sense of anticlimax. The words of one of the contributors – ' the experiments raised as many questions as they answered ' – could be applied to most of the activities, and it was particularly disappointing to see indecisive results on such matters as the relation between eddy diffusivities. Such experience must open up the whole difficult issue of how progress is best achieved in the present field – by a comprehensive effort on a general front or by concentration on a particular and limited question ?

The volumes are well produced but the presentation contains some minor flaws which temporarily confuse – for example the interchange of Figs. 7.54 and 7.55. For the set of two volumes the price of £7 is high, but this is presumably inevitable in a publication which is for the specialists and the reference libraries.

F.P.

' Meteor ' Expedition, 1925-1927, Bd. 6, Teil 2. By G. Wüst. Berlin (W. de Gruyter), 1957. Pp. 261-420; Figs.; Tables. 54 DM.

This long paper (30 cm × 23 cm) richly supplied with diagrams, illustrative chartlets, and no fewer than 34 Tables, is a very worthy addition to the fine series of earlier reports resulting from the justly-renowned ' Meteor ' Expedition which took place a third of a century ago. Its author, Professor Georg Wüst, now Director of the Oceanographical Institute of Kiel University, was himself a very active member of the expedition, and his earlier reports on the work done are well known to oceanographers. The present report deals with the mass water movements in the depths of the Atlantic Ocean and with the speeds of the currents there, and has particular regard to the deep and bottom waters. Wüst has laboured greatly over a long period of years to present this detailed interpretation of the ' Meteor ' sections in terms of transport and speed. The reviewer has been told that during the period 1953 to 1956, some 150-200,000 computations were made in the Kiel Institute to produce the values needed for the present dynamical study.

The report would in any case have been an oceanographical publication of first importance, but has a particular topicality because of certain absolute determinations of water-movement speeds made at great depths in the Atlantic by Swallow, using his neutral-buoyancy float technique. Then too, there is all the present-day attention (within the ambit of the I.G.Y. programme) to deep-water movements, not only in search of knowledge for its own sake, but also in connexion with the need to discover where radio-active wastes could least-harmfully be jettisoned.

Wüst produces some fairly convincing arguments that there are three levels of minimum motion in the South Atlantic. The cold and salt-poor Antarctic bottom water has a northwards component; the great mass of water at mid-depths and of salinity just in excess of 34·9 parts per thousand moves southerly; the least saline layer with its axis 700 m deep moves north, whilst, nearer the surface, there is a southwards component.

The directions of movement of the different layers revealed by Wüst's new study occasion no surprise, but the same cannot be said of the rates of flow which emerge from his calculations. Some, e.g. 17·4 cm/sec

at 3,500 m depth in the axis of the North Atlantic Deep Water Current, and 12·5 cm/sec both at 3,750 and 4,000 m depth in the axis of the Antarctic Bottom Current are of such magnitude as to give special point to the hope that it may soon be possible to use the Swallow technique at critical points in these South Atlantic profiles. It is known that Wüst finds great satisfaction in the almost exact correspondence of the speeds (23 mi in 66 hr) measured by Swallow in the south-moving North Atlantic Deep Water under the Gulf Stream, and the value of 17·4 cm/sec which he (Wüst) computed for the speed of the same water only several degrees north of the equator. Since Wüst's computations depend greatly upon his use of ' Defant's reference level,' which is the depth at which little or no horizontal movement should exist, but above and below which the water should move north and south respectively, they derive noteworthy support from the fact that Swallow's results beneath the Gulf Stream reveal a level of no movement between 4,500 and 6,000 ft depth with pronounced southgoing flow beneath. The mass water movements having been computed by Wüst across the closed ' Meteor ' sections VIII, VI, II and IV, he found in the volume transports involved a quantitative explanation of the stratification differences existing as between the opposite sides of the Mid-Atlantic Ridge. Furthermore his transport computations, based upon acceptance of Defant's reference level, surprised him by revealing the existence of two additional levels of negligible or nil motion nearly corresponding with the depth boundaries between the water masses of the North Atlantic Deep Current, the Antarctic Bottom Current, and an Antarctic Bottom Counter Current present in the greatest depths of the Argentine/Brazil Basin.

Wüst's results set out in this very valuable report show very markedly that in the Atlantic Ocean between 5°S and 35°S there is a westwards intensification of the water exchange which puts its stamp upon the stratification across the whole breadth of the ocean.

During recent crossings of the Atlantic Ocean by the Woods Hole research vessel *Crawford*, stations in 16°S latitude were worked at closer intervals (33 stations instead of 22) than on the ' Meteor ' Expedition, but the inferences as to transport would be virtually the same.

Despite great differences in sense and volume as between the transports on opposite sides of the mid-Atlantic Ridge, the overall N-S balance across the full width of the ocean should, of course, be zero. It is accordingly of great interest to see that (in units of 10^6 m^3 sec^{-1}) the computed southerly transport between 1,400 and 4,000 metres depth (25·6) tallies almost exactly with the sum of the values 22·8 and 3·0 which relate respectively to north-moving water above and below.

J.N.C.

The threshold of space. Ed. M. Zelikoff. London (Pergamon Press), 1957. Pp. xi, 342; many Figures and a number of Plates. £5 5s. 0d.

The title of the book is presumably a concession to the Satellite Age, but it is a pity, for the contents are far more scientifically satisfying than the title suggests, and we must consult the sub-title to find that this is another conference report by the Pergamon Press, this time ' The Proceedings of the Conference on Chemical Aeronomy ' held at Harvard in June 1956.

45 papers by authors of the highest distinction are grouped under four headings.

I. Atmospheric photochemistry – Earth and Venus.
II. Spectroscopy and photochemistry.
III. Rocket probing of the upper atmosphere.
IV. Phenomena produced by hypersonic flight.

The titles suggest, and the text confirms, that the book contains a curiously mixed bag, but one of interest to the upper-atmosphere specialist. For the general reader, however, the book will not have great appeal, for papers of similar quality are to be found in standard journals, and something rather special is required to justify the high price. Nevertheless, the price is an improvement on recent similar publications, and it is hoped that the improvement will continue. To offer a concrete suggestion, omission of the verbatim discussion would effect some economy and at the same time spare the participants embarrassment and the reader irritation.

Section III is the most interesting, for it seems that this subject has suddenly come of age, and instead of merely confirming existing ideas, rocket research is now providing authoritative answers to some of the outstanding problems of the airglow.

Minor errors and other blemishes are too many to detail, and some of the writing is very poor. A price must, of course, be paid for rapid publication, but it should not be difficult to improve on this position.

R.M.G.

Light scattering by small particles. By H. C. van de Hulst. New York (John Wiley and Sons), London (Chapman and Hall), 1957. Pp. xiii, 470; 103 Figs.; 46 Tables. 96s.

Those engaged on analysing observations of light scattered from concentrations of small particles, sooner or later come up against Mie's solution of Maxwell's equations. If they are fortunate they will find that tables exist for the solutions required in their particular application ; if not, then a massive calculation must be undertaken, or else the investigator must use ingenuity in deriving an approximate answer. To those

faced with such a choice, this book is a valuable aid. The formal Mie solution of Maxwell's equations is summarized in a clear form suitable for those intending to compute their answer, but most of the book is taken up with the special forms which the solution assumes in particular cases, involving the various stages of relationship of wavelength to particle size, and the different conditions, shape and type of particle.

This is not a book to be read from cover to cover, but is rather a handbook to which one can refer for help with a particular problem. The author also seems to regard this as the function of his book and has the contents well sign-posted so that the reader can quickly find his way to the formula he wants, or else is directed outside, to where the information may be obtained, by means of up-to-date references at the end of each chapter.

The last four chapters deal with the types of problem in which light-scattering theory is applied, either as a means of explaining natural phenomena, or to obtain estimates of the magnitude of particle size, concentration and refractive index. One chapter each is devoted to applications in meteorology, chemistry and astronomy.

The author has made a special study of light-scattering problems since his first treatise on the subject in 1946, and this book can be regarded as a more comprehensive and up-to-date version of that earlier treatise, which is already well known, and which has been the spring-board for many an investigator into problems on scattered light.

The book is well produced and the reviewer has noticed only one printing error, in the omission of a power-of-two figure in one of the formulae on p. 124. However, it would be surprising if there were no slips in a book which contains some mathematics on nearly every page.

The diagrams are clear and well constructed, many of the graphs being of direct use in applications of the theory : the Tables, which, the publishers inform us, are mostly new, also contain information of direct utility.

One slight criticism that can be made is that the English narrative sometimes takes on a ' continental ' construction, which more careful editing might have avoided.

D.D.C.

Atmosphärische Elektrizität, Teil I, Grundlagen, Leitfähigkeit, Ionen (Probleme der kosmischen Physik. Band XXIX). By H. Israel. Leipzig (Akademische Verlag., Geest & Portig K-G), 1957. Pp. ix, 370. 124 Figs.; 16 Tables. 42 DM.

In this, the first part of Dr. Israel's long-awaited book on atmospheric electricity, he has given us what can best be described as an encyclopaedic work. There can scarcely have been a paper published in the branches covered that is not at least given a reference, and there are about 700 separate references, complete, one is pleased to notice, with the titles of the papers, even though these have occasional misprints.

After a historical introduction, there is a section on electrical conduction in gases without special reference to atmospheric electricity; then follows a general survey of the whole of atmospheric electricity. The largest part of the volume is taken up by the fourth chapter on the conductivity of the atmosphere and its causes. Finally, there is a long appendix on measurement techniques, followed by some useful tables.

Dr. Israel has brought together in a convenient form a great deal that otherwise would have to be sought in original papers and it may seem ungrateful to be critical ; but there are several matters which the reviewer would have preferred to see treated differently. Most of the workers in atmospheric electricity are now using the MKS systems of units and it is to be regretted that Dr. Israel has not done so. Table 7 on p. 88 appears to suggest that the Actinium series is somehow directly descended from the main Uranium series, the isotope Uranium 235 not being mentioned ! The emanation of Thorium is given sometimes as Thoron and at other times as Toron. The statement, on p. 98, that a total negative charge on the earth does not exist, is not one which would be generally accepted, at least without more discussion. In view of its importance in the discussion of conditions near the earth's surface, one might have expected a fuller account of the work of Hogg on the variation of polar conductivity with height. On the other hand perhaps too much attention is given to the discussion of ionic equilibrium, when it is known that this seldom if ever is reached in the free atmosphere.

These, however, are only minor points of criticism of what is undoubtedly a most useful contribution to the literature of the subject and one which no worker in it will do without. It is probably too complete to be suitable for the beginner.

We can look forward to the second volume on fields, charges and currents, but if it is to be as complete as the present volume, it would undoubtedly be much larger.

Finally, the English reader whose German is not very strong will thank Dr. Israel for his direct style and his avoidance of the involved constructions so frequently found in German scientific literature.

J.A.C.

Vom grossen Eiszeitalter. By E. Ebers. ' Verständliche Wissenschaft ' Series, Bd. 66. Berlin (Springer), 1957. Pp. 138; Photographs; Maps; Diagrams, Index. 7.80 DM.

In this post-war era the cheerful progress and orderly accomplishment of the scientist has provided for many an agreable antidote to much of the prevailing *Weltschmerz*. In all countries there is a widespread demand for short, well-written summaries of established knowledge. The compact little work under notice

is a decidedly pleasing example of its kind, and does credit to its authoress and its publishers, although it is considerably more expensive than its equivalents in Britain or France. Beginning with an account of the snowline and of glaciers, and of the evidence for greater glaciation in the past, it is understandably addressed primarily to German readers and hence most of the examples and references are drawn from Central Europe. English readers will however notice the up-to-date representation of the limits of our glaciation, and good illustrations of the Cromer Forest Bed and of Ullswater. Many of the maps will be found useful, e.g., the reproduction of Büdel's vegetation map. Admirable summaries are given of much of the earlier Scandinavian work; a neat little series of maps illustrates Sauramo's views of the evolution of the Baltic; the value of pollen-analysis is crisply indicated. The Köppen-Milankovitch scheme is described and a useful reminder is provided that a variety of alternative explanations have been put forward. There are compact chapters summarizing the effects on plants, animals and man and the methods of dating. English university students requiring a quick sketch of a wide field of knowledge will agree that their German contemporaries are very well served.

G.M.

Precipitation, temperature and sultriness in Africa. By K. Knoch and A. Schulze. Hamburg (Falk Verlag), 1957. Pp. 6. 6 large and 6 small maps. Np.

This volume, whose text is in both German and English, contains a set of climatic charts originally published in the *World Atlas of Epidemic Diseases*. The charts are all for the whole of Africa. There are six coloured charts on a scale of 1 : 20,000,000 giving mean actual January and July temperatures (2°C isotherms), mean annual precipitation (500 mm interval isohyets), months containing the rainy (monthly rainfall over 125 mm) and dry seasons, and the 'mean annual thermic sultriness.' The temperature maps are for actual values not reduced to sea-level with the result that they are to a large extent a reflection of the topography. They are of use for giving an overall general picture of the temperature distribution, but are not satisfactory for interpolation to find the values at individual places in hilly or mountainous areas. They are admittedly based on observations of varying lengths and periods of time. The thermic sultriness value (°C) is that temperature at which for the mean relative humidity the water vapour pressure would be 14·08 mm Hg minus the actual temperature. Negative values correspond to sultriness. This depends on an observation by Scharlau that air having a vapour pressure exceeding 14·08 mm feels 'sultry.' This gives high 'sultriness' in the hot damp equatorial and central coastal areas and the 'opposite' of 'sultriness' in the desert areas. If the thermic sultriness value is positive, conditions are said to be comfortable with the qualification that 'comfort' is at a maximum for + 7°C and decreases at higher values. In addition there are maps of mean actual annual temperature, annual range of temperature, prevailing winds and air masses in January and July and the months of mean thermic sultriness below 0°C and below − 6°C.

G.A.B.

Solar control and shading devices. By A. Olgyay and V. Olgyay. Princeton University Press (London : Oxford University Press), 1957. Pp. 201; 180 Figs.; Numerous plates. £5.

The successful design of a solar shading device ultimately depends on basic data supplied by the meteorologist. Nevertheless the incorporation of these data in design techniques capable of being used by non-specialist architects demands the skill of persons who are well versed in both solar climatology and architecture. The Olgyay brothers meet this dual requirement, being practising architects who have made a special study of sun control and its scientific application.

Their book deals first with the architecture of shading devices. Various technical aspects determining the ultimate form of these devices are then considered. The design technique which they advocate involves choosing a control device of appropriate geometry to intercept the sun's rays during those periods when overheating would otherwise occur (these periods being determined by the times of day throughout the year when the temperature is above 70°F at the location in question). The book concludes with brief descriptions, photographs and scale drawings of over one hundred actual applications of solar control devices, each being accompanied by a sky vault projection on which is superimposed the shading of the particular device concerned.

The authors give no information on how successful their design technique has proved in practice. It would be of value to know this, since the technique does not ensure complete exclusion of the sun, and sun penetration may not always be acceptable during the underheated period. In addition, the usefulness of the projections accompanying the applications would have been enhanced if sun paths and overheating periods had also been superimposed, so that the reader could judge how much individual designs were determined by 'architecture' and how much by functional requirements.

The book should prove of the greatest value to architects concerned with the design of buildings for warmer and more sunny climates than our own. It is unlikely, however, to be of much more than academic interest to those concerned with the design of buildings for this country, since the problem of sun penetration is relatively slight and because our generally low levels of daylighting over a large part of the year do not encourage the use of permanent shading devices.

P.P.

Obituary Notices

Mr. C. R. ARMSTRONG, B.Sc., L.D.S., F.R.A.S.

Christopher Rodney Armstrong, a Fellow of the Society since 16 February 1955, died suddenly on 5 November 1957. After the 1939-45 war, during which he served in submarines, Mr. Armstrong graduated B.Sc. at the University of Manchester. On coming south for the study of both medicine and dentistry at Guy's Hospital he became a Fellow of the Royal Astronomical Society and also joined the British Interplanetary Society. He had taken three years of his M.B. and B.S. course at Guy's when he transferred to dentistry. He obtained his L.D.S. in June 1957 and at the time of his death had just returned to complete his medical training. His enthusiasm for research was made evident by his assistance to a number of his tutors in original work, which included investigations into the growth and structure of crystals. With the approval of the Hospital he also conducted research of his own into certain aspects of dental growth. Among his ambitions was the ultimate undertaking of research in the new field of ' space ' medicine.

P.R.F.

Mr. J. H. PARRY-JONES, M.A., A.F.R.Ae.S., F.B.I.S.

John Harold Parry-Jones, a Fellow of the Society since 19 March 1952, was killed in the crash of the Britannia aircraft at Downend, Bristol, on 6 November 1957. Born in 1917, he received his education at Rossall School and Brasenose College, Oxford. After graduating, he joined the Bristol Aeroplane Company in 1940. Mr. Parry-Jones was one of our many Fellows whose interest in meteorology grew out of enthusiasm for engineless flight. He had served as chief flying instructor to the Bristol Gliding Club, and at the time of his death he was its vice-chairman.

E.L.H.

Dr. A. J. BAMFORD, M.C., V.D., M.A., Ph.D.

Dr. Alec Joscelyne Bamford died at Worthing, Sussex, on 19 November 1957, after a brief illness. He was in his 73rd year. Dr. Bamford was educated at Malvern and at Emmanuel College, Cambridge. After graduating as tenth wrangler (bracketed) in 1907, he took up in November 1908 a post as assistant at the Colombo Observatory. In January 1913 he became Superintendent of the Observatory. Dr. Bamford was responsible for many improvements in the work of the Observatory. Among these were the initiation of a programme of pilot-balloon observations and of a radio time service for the benefit of shipping. He also investigated the possibility of forecasting flood data for the Kelani River at Colombo, from the rainfall figures reported daily in the Kelani Valley, and devised a method of obtaining these forecasts, which was adopted officially for flood warnings at Colombo.

Dr. Bamford carried out many investigations, chiefly on the meteorology of Ceylon, and published his results in scientific journals in Ceylon and Europe. In addition to his Observatory duties, on several occasions he acted as Professor or Lecturer in Physics at the University College, Colombo. During the 1914-18 War Dr. Bamford served with the armoured cars in German East Africa, and was later employed on sound ranging and army survey work in Palestine and Arabia. He received the Military Cross, and was twice mentioned in despatches. In 1919 he was demobilized with the rank of Captain. Throughout his service in Ceylon, Dr. Bamford took a great interest in the Ceylon Planters' Rifle Corps. Joining as a rifleman he retired from the Corps in 1931, on his final departure from Ceylon, as a Major.

On his return to England, Dr. Bamford was for some years engaged in teaching, but in 1941 he accepted a post in the Meteorological Office, and was posted to the Marine Branch, which had then been evacuated to Stonehouse, Gloucestershire. There he was mainly engaged on the principal war-time activity of the branch, the production of meteorological atlases covering all the oceans of the world. Both before and after his arrival at Stonehouse, Dr. Bamford served with the Home Guard. He retired from the Meteorological Office in 1946, and moved to Ferring, Sussex.

Dr. Bamford was elected a Fellow of this Society on 19 June 1912. At one period he served as a Member of Council. His contributions to the proceedings included papers on the rainfall of Ceylon, on upper-air observations over Palestine, and on an anemometer of his own devising.

H.J.

Dr. I. C. BROWNE, M.A., Ph.D.

Ian Campbell Browne died suddenly on 8 December 1957, at the age of 30. He was educated at University College School, London, and Magdalene and Trinity Colleges, Cambridge. His Ph.D. dissertation at Cambridge was concerned with a study of processes occurring in rain clouds, chiefly by radar methods. From 1952 to 1955 he was a member of the staff of the Jodrell Bank Experimental Station of the University of Manchester, where the carried out radio-echo studies of meteors and of the moon. In the latter field he contributed to the development of an important new technique for the measurement of the total ionospheric electron content. After a period as assistant master at Marlborough College, Dr. Browne was appointed Lecturer in Physics in the University of Sheffield in January 1957. He joined the newly formed Radio-astronomy Group and up to the time of his death he was actively engaged in the development of a programme of investigation of the aurora and of geomagnetic disturbances.

Ian Browne was an able and conscientious teacher as well as a talented research worker in the field of atmospheric physics; he was equally at home with problems concerning the lower and upper regions of the atmosphere. His tragic death at such an early age terminates a career of great promise and is felt keenly by his students and colleagues.

Dr. Browne was elected to Student Membership in this Society on 15 December 1948 and proceeded to Fellowship on 17 December 1952. He had served on the Council since October 1957. The first of his contributions to the Society's publications, a paper entitled 'Precipitation streaks as a cause of radar upper bands,' appeared in the *Quarterly Journal* while he was still a Student Member.

T.R.K.

Dr. C. E. P. BROOKS, I.S.O.

By the death of Dr. Brooks on 14 December 1957 climatology lost one of its best-known and most enthusiastic exponents. Charles Ernest Pelham Brooks was born in Central London on 10 November 1888. In 1903 he won a scholarship to University College School, where he was entered on the commercial side. Though this gave him little opportunity for receiving mathematical instruction, it did not prevent him from showing at that early age his natural mathematical ability by winning the school's mathematical prize. On leaving school in 1907, he took a probationership in the Meteorological Office; then by work in his spare time he took, in 1912, a degree with honours in geology. In 1916 he gained the M.Sc. in geology and in 1926 the D.Sc. in meteorology. The paper on which the last-mentioned was awarded was 'The variations of pressure from month to month in the region of the British Isles' – the first of a series of investigations on the problem of seasonal forecasting, to which reference is made again later in this notice. In 1913, with three other honours graduates – direct entrants – he was appointed to the newly established grade of Junior Professional Assistant in the Meteorological Office. In the same year he became a Fellow of the Royal Meteorological Society and read his first paper. This, 'The meteorology of an ice sheet,' was the first of many for he contributed over 60 original papers (some as part author), as well as many reviews, translations and commentaries, to the *Quarterly Journal* alone. He was awarded the Buchan Prize of the Society in 1931. He was a Councillor from 1916 to 1921, from 1923 to 1926 and from 1936 to 1938, Honorary Secretary from 1927 to 1931 and Vice-President in 1932 and 1933. For a long period up to 1948 he was the Society's librarian and, in 1936, he introduced into the library the Universal Decimal Classification for meteorological literature, as approved by the International Commission (Warsaw, 1935), of which he had been a member.

Of his work in, and for, the Meteorological Office, it would be almost impossible to speak too highly. The complete record also would be a long one. His studies of world climate, in particular of the world distribution of thunderstorms and of the changes over the world in geological time, are well known. He was intimately associated with the compilation of the first 25 years of the *Réseau Mondial* in fulfilment of the plans of Sir Napier Shaw. The great library of the Office is organized on lines largely planned by Brooks and it is doubtful if there exists anywhere a specialized library so well indexed from the point of view of the requirements of practical inquiry or research. He represented the Meteorological Office at various international meetings, Wiesbaden in 1934, Warsaw in 1935, and was a member of the Commissions of the International Meteorological Organization for Climatology and for Bibliography and Publications at Toronto in 1947. For 22 years he edited the *Meteorological Magazine*. In 1948 he was created a Companion of the Imperial Service Order. He retired from his appointment as an Assistant Director of the Meteorological Office in November 1948, but continued to assist the Office in a part-time capacity for a year or two thereafter.

In Brooks's scientific work, as he himself said, his first love was geology; then this was mated with meteorology and the offspring was naturally palaeoclimatology, or the study of climatic changes in geological time. In his first book, *The evolution of climate*, published in 1922, he tried to collect and summarize existing knowledge, reaching the conclusion that 'owing to interactions between different regions, great climatic changes such as glaciations must be contemporaneous – geologically speaking – in all parts of the world.' In the next stage of this work – a series of papers in the *Quarterly Journal* – and in his book *Climate through the ages* (1926, revised 1949) he reached two main principles, first that all the major changes of climate, lasting millions of years, could be explained solely by 'geographical' causes – land and sea distribution, relief, ocean currents, volcanic activity, etc. – leaving only fluctuations of, say, 100,000 years and less for variations of solar radiation, eccentricity and similar factors; and secondly, that there is a great gulf between 'glacial' climates such as Ice Ages and the mild or non-glacial climates which have made up most of geological time. These conclusions he was able to establish by numerical evaluations and estimates of the magnitudes of geographical factors in geological ages in comparison with quantitative evaluations made from the geological evidence alone. The study of climatic changes within the historical period also received his attention, the results having been published in various *Geophysical Memoirs* of the Meteorological Office and in papers in the *Quarterly Journal* of the Society. In the course of his official duties, numerous series of meteorological observations in various parts of the globe came under Brooks's notice. Many of these he discussed and analysed, presenting the results in accessible form. Very often in his investigations he was assisted by members of his staff, infected by his own enthusiasm. Brooks was author or part author of eight books and something like 150 articles in journals other than the Society's. Among his papers, mention may be made specially of a series connected in various ways with possible methods of forecasting over months or seasons; for example, 'Regression equations with many variates' (*Prof. Notes*, No. 47), 'Sunspots and

distributions of pressure over western Europe' (*Prof. Notes*, No. 49) and 'Variations of pressure in the northern hemisphere 1904-13 based on 10-day means' (*Prof. Notes*, No. 54). Mention should be made also of the valuable series of reports which he prepared for the International Commission appointed to further the study of solar and terrestrial relationships. A summary of the literature on this subject up to 1914 had been given by Nansen and Helland Hansen in 1920. Brooks's reports of 1926, 1936 and 1939 brought this up to 1938 and a report of 1950 contains a discussion of the position and such conclusions as could be stated at that time.

Towards the end of the 1939-45 war, Brooks with C. S. Durst and others felt that the charting of upper winds over the globe was becoming a matter of great urgency, not only from the viewpoint of aviation but as being essential material for meteorological research. Observational data were, however, very unevenly distributed over the world and many areas were not represented at all. The only course open therefore was seen to be to develop the available data to the best advantage by statistical study and to work out generalizations by means of which the most could be made of the sparse data and – in the absence of data – the most appropriate extrapolations might be reached. A preliminary paper in the *Quarterly Journal* of 1946, 'The frequency distribution of winds at a point in the free air,' gave an outline of methods in view and the final result appeared in 1950 in *Geophysical Memoir* 85, 'Upper winds over the world.' Brooks's last major work for the Meteorological Office was the preparation (with N. Carruthers) of the *Handbook of statistical methods in meteorology* (main text completed 1949, published 1953), a comprehensive work which has been well received and is used by research workers in other observational sciences as well as in meteorology. After his official retirement he did a great deal of work for the American Meteorological Society. He also completed *The English climate* and wrote numerous articles for the press.

Mentally and physically, Brooks was a man of surprising energy and endurance. Bridge and chess were among his indoor recreations and for many years he played first board in the Air Ministry chess team. He was a most loyal friend and colleague. The trials of dispersal during the second world war, the wearisome journeys between Stroud, Dunstable and London on official duties, were all taken in good spirit, and the good of his staff and the work were always his first concern.

In 1916, Brooks married Miss Dora Buckeridge who was also at that time on the staff of the Meteorological Office. She gave him much assistance in his work. He is survived by his wife and one son.

At the funeral held at the Downs Crematorium, Brighton, on 17 December, the Royal Meteorological Society was represented by Mr. R. G. Veryard, Vice-President.

A.H.R.G.

Sir ALEXANDER GIBB, G.B.E., C.B., LL.D., F.R.S.

Sir Alexander Gibb, the eminent consulting engineer, died at his home in Hartley Wintney on 21 January 1958, at the age of 85. Over the long period of 200 years his family have been Civil Engineers, and he was the fifth in succession from father to son, starting with his great-great-grandfather, William Gibb (1736-1791), whose son, John Gibb (1776-1850), was associated with Telford. John's son, Alexander, was a pupil of Telford's and his son, Alexander Easton, (1841-1916), was carrying out work in Scotland when Sir Alexander was born at Broughty Ferry, Angus, on 12 February 1872.

Sir Alexander was thus born into engineering and after his schooling at Rugby and University College, London, he became a pupil of the late Sir John Wolfe Barry and H. M. Brunel. He remained with them for a few years after serving his pupilage and then joined his father as a contractor in the firm of Easton Gibb & Son Ltd. They carried out a number of large engineering works, the most important of which was the Rosyth Naval Base. Sir Alexander's war service in the 1914-18 war was first of all as Chief Engineer for Ports Construction to the British Armies in France and Belgium, and afterwards as Deputy Director of Docks. In 1918 he became Civil Engineer-in-Chief at the Admiralty, being the first holder of that appointment, and in 1919 he was Director-General of Civil Engineering to the Ministry of Transport. In 1921 he entered private practice and in 1922 founded the firm of Sir Alexander Gibb and Partners, Consulting and Civil Engineers. From these small beginnings there grew up, under his leadership, this firm, that soon gained a world-wide reputation in all fields of civil engineering.

Sir Alexander's connection with a meteorological society dated from 1912, when he became a Life Fellow of the Scottish Meteorological Society. In 1921 this society amalgamated with the Royal Meteorological Society and Sir Alexander was transferred.

Sir Alexander's Life Fellowship was perhaps symbolic of his recognition of the part that the work of the Society plays in so many fields of civil engineering. The engineer depends much for his success upon his ability to predict the behaviour of the elements. As the prediction of climatic changes and trends came to be more of a precise science, he was quick to realise the advantages that could be derived from familiarity with the latest methods. Examples of this constantly came up in the field of hydroelectric engineering, in the development of water resources and in the control of rivers, the salinity of estuaries and siltation, the control of floods and the relief of droughts. He was particularly interested in the harnessing of the elements for the benefit of mankind : he did not regard them as essentially man's enemies, but thought it was up to the engineer to understand them and thereby to benefit from them. These ideas may seem commonplace nowadays, but Sir Alexander was one of those who helped to make them so.

At the memorial service for Sir Alexander Gibb, held at the church of St. Margaret, Westminster, on 18 February 1958, the Royal Meteorological Society was represented by Mr. J. F. Shipley.

M.E.G.

Dr. J. S. FARQUHARSON, M.A., D.Sc.

Driving through fog on the way to his Dunstable office on Friday, 31 January 1958, Dr. James Strachan Farquharson, Honorary Treasurer of the Society and Life Fellow since 1934, met with a fatal motoring accident and British meteorology lost a prominent and colourful personality, the Society a conscientious officer, and his colleagues a warm-hearted friend.

His career, after graduation at Aberdeen University, although entirely within the organization of the Meteorological Office, and concerned with forecasting or forecasting services, was not the steady unexciting progress popularly identified with a Civil Service life. His first ten years were mainly spent at Headquarters in London and at the airport of Croydon but from 1935-6 he was with the Royal Air Force in the Sudan, in 1939 in Paris as liaison officer with the French Meteorological Service, having been gazetted Squadron Leader in the R.A.F.V.R., in 1940 in Iraq as Director of the Iraqi Meteorological Service, in 1942 in India. In 1943 he became, as Wing Commander, Senior Meteorological Officer, 2nd Tactical Air Force and was finally demobilized in 1945 after serving on the Continent with 2nd T.A.F. and S.H.A.E.F. almost to the end of hostilities. There was then a period as Chief Meteorological Officer, Bomber Command before the move to Dunstable as one of the chief forecasters, a post which was held for six years. In 1954 Farquharson was once more selected for a new post requiring special aptitude, as Assistant Director for Public Services, which brought him into close contact with broadcasting authorities and personally before the microphones and cameras on a number of occasions. Both the television weather broadcasts and the automatic telephone weather services, which seem likely to become permanent features of the British way of life, owe much to his direction. In his last year he had started yet again on new responsibilities, this time in charge of communications and observations.

Throughout his career Farquharson was above all an enthusiast for his profession, bringing to every new task a fresh outlook and a reserve of energy. His opinions were essentially original, he thought deeply, was prepared to take on any worthwhile task and to carry the responsibility. It is perhaps not surprising if he appeared to have the faults of his virtues and, to those on whose corns he inadvertently trod, seemed unpredictable, impetuous or at times tactless.

Dr. Farquharson never held a research post but was always deeply interested in the scientific as well as the organizational problems of his profession and his contributions to the *Quarterly Journal* in 1937 and 1938 on the diurnal variation of wind in tropical Africa and on Haboobs and Instability remain useful references. His close interest in modern meteorology is shown by a recent research paper on vorticity (not otherwise published). Up to the time of his death he was working at home on meteorological dynamics and on the Russian language, the latter to add to the French and German which he already used fluently.

I suppose that as Treasurer of the Society he must be regarded as orthodox, for where finance was concerned he had a Scottish sense of purpose and never failed to point to the consequences of any proposed extravagance; yet it was he who successfully advocated the relaxation of the invariable gilt-edged rule for the Society's investments and so introduced a measure of flexibility which may prove valuable in years to come.

As one who first met Farquharson as a colleague, some thirty years ago, and who has enjoyed his friendship and often close companionship over these many years, I can think of no one of his generation who gave more of himself to his profession. At the time of his death he seemed to have seen a way to channel his energies even more rewardingly. It was an unfinished career but much was accomplished.

R.C.S.

Corrigendum to October 1957 issue

G. de Q. Robin—

p. 536, Table 3, column 4, 4th row from bottom—

for 29 – 26 *read* 29 – 66

Note: This figure is quoted from 'Upper winds over the world,' *Geophys. Mem. Met. Off.*, No. 85 by C. E. P. Brooks *et al.*, being the March-May mean vector wind at 500 mb at approximately 70°N, 50°E. Later and more reliable estimates of the April wind at 70°N, 50°E (e.g., *Air Ministry. Met. Res. Cttee.*, M.R.P. No. 1020 by H. Heastie) give a value of 27 – 30.

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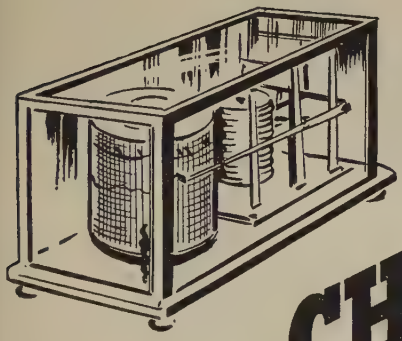

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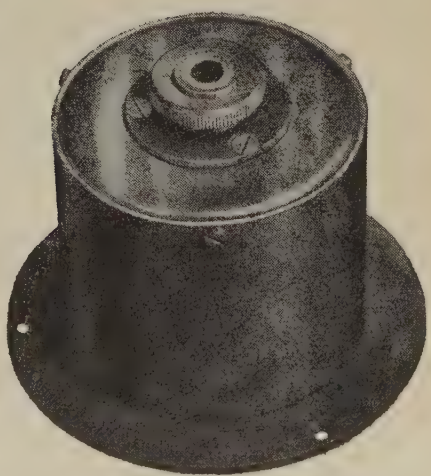

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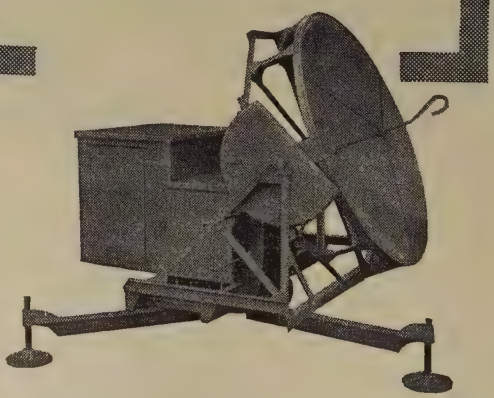

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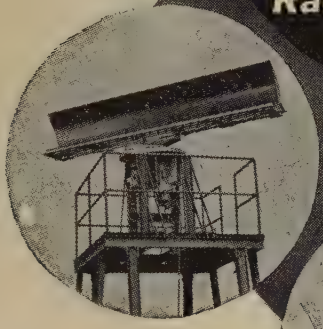

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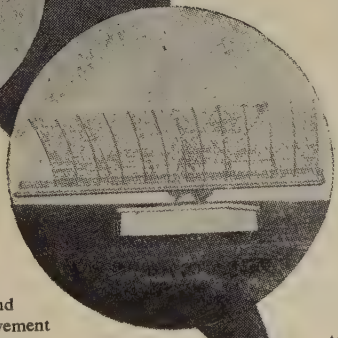

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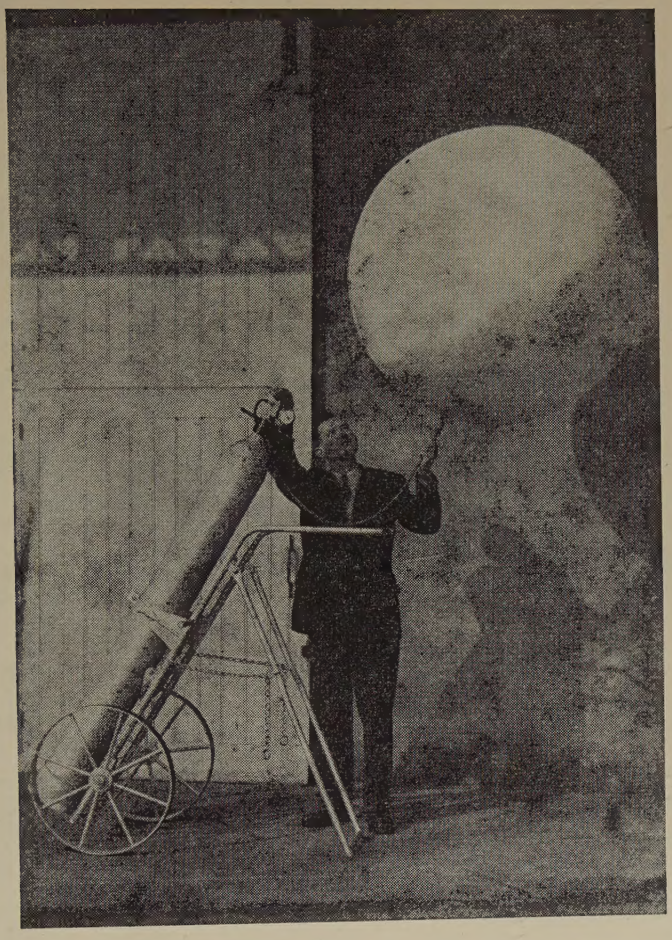

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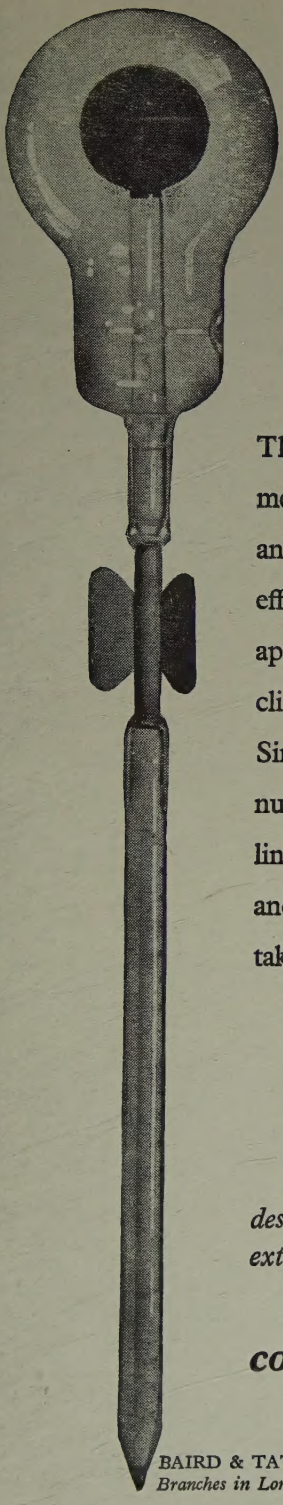

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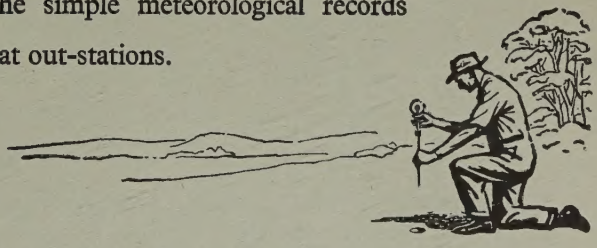

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